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## I.—WHAT WAS ICTUS IN LATIN PROSODY?

English poetry, as a rule, is based on stress, i. e. on a regular succession of stressed and unstressed syllables grouped by twos or threes. The versification of

This is the forest primeval, the murmuring pines and the hemlocks, depends entirely upon this harmonious alternation of stressed and unstressed syllables, and the same is true of all ordinary English verse.<sup>1</sup> This basis of English poetry, moreover, inheres in the very nature of the English language. Like all languages of the Germanic group, our English speech is strongly stressed; we pronounce our words with an energy typical of the race.

Latin verse, on the other hand, like Greek, was based on quantity. Recent discussion, it is true, has tended to show that the native Latin verse, as exemplified by the Saturnian measure, was governed by stress; but however that may have been, it is certain that, from the time Greek metres began to be introduced at Rome,—from the time of Ennius,—Latin verse was quantitative like Greek; a line of Latin poetry consisted of an orderly and harmonious arrangement of long and short syllables, i. e. of syllables which it took a long or short time to pronounce. This basis of Latin poetry again, as in the case of English poetry, is strictly in conformity with the character of the spoken language. For Latin apparently, in the classical period, was not a strongly

<sup>1</sup> Such exceptions to this principle as occur (see, e. g., Goodell, *Transactions Amer. Phil. Assoc.* XVI, p. 78 ff.) hardly concern the purposes of the present discussion.

stressed language.<sup>1</sup> Had it been, it is quite inconceivable that the long environing vowels should not have been shortened in such words as *evitabatur* and scores of others like it in which the Latin language abounds. Cf., e. g., a Latin *inevitabile* with English *inevitable*. Strong stress necessarily reduces every long pre-tonic and post-tonic syllable to a short one. In other words, strong stress is absolutely inconsistent with the quantitative phenomena of the Latin language.

This view of the Latin accent is further confirmed by the status of the Romance languages. So far as my information and observation go, no one of these languages is strongly stressed. In the utterance of French and Italians, I often find it difficult to determine on what syllable the stress rests,—so relatively slight is it. In fact the quantitatively monotonous character of Italian makes a strong stress accent impossible. This consensus in accentual character on the part of so many daughters of the Latin, while it cannot be held to amount to a proof that Latin was but slightly stressed, nevertheless seems to me to point distinctly in that direction. In fact I feel at times inclined to go much further and to maintain that the Latin language of the classical period was absolutely unstressed. As such a thesis is easily defensible, and as its consideration may throw light on the subject under discussion, I shall venture to advocate this probably startling view.

The thesis is that the Latin language of the classical period was unstressed. I do not say unaccented, for that would be to contradict the express testimony of the whole apostolic succession of grammarians from Varro to Priscian; but I do say unstressed. The Roman grammarians tell us that a long penult was accented; they tell us, further, that the antepenult was accented when the penult was short. Now, what was this accent, this *accentus* of the Roman grammarians? Or first, what is accent in general? Accent is an elastic term. To a person familiar with English only, accent seems a very clear and simple thing; naturally so because such a person imagines that all languages are like his own; hence he will answer that accent is exemplified by the second syllable of *potato*, or the final word of the phrase *let her go!*, i. e. by accent he understands stress. But the study of comparative phonetics teaches us that there are languages in which the stressing plays no such rôle as in English and in the Germanic languages generally, and that in some of these other

<sup>1</sup> Cf. Sievers, *Grundzüge der Phonetik*<sup>2</sup>, §35, 1, Anm. 1.

languages the chief feature of the spoken idiom is the rising and falling of the voice. Certain syllables are thus uttered at a higher pitch than others, and it is this shifting of pitch that gives the character to the speech. According to the commonly accepted view, Greek was characterized by this kind of utterance, and the syllables whose pitch varied from the ordinary were designated in accordance with principles to which we give the name of accent. Sanskrit also at one stage, it is thought, must have been so pronounced. In these two languages, therefore, what we call accent was primarily variation in pitch.

We have already recognized two senses in which the word accent is used. Both, while radically different, have at least this much in common: The 'accented' syllable is the one made prominent in some way in oral utterance.<sup>1</sup> When the word is spoken, that syllable stands out conspicuous, either by virtue of its stress, i. e. a definite expulsive effort of the lungs, or by virtue of its pitch. To these two varieties of an accented syllable, i. e. a syllable standing out prominently in an uttered word,—to these two conceptions, I ask, may we not add a third? May not a syllable be primarily prominent by virtue of its *quantity*? That is, in a word like *amāvit*, for example, may not the rule of the grammarians, that such a word was accented on the penult, simply mean that they felt the quantity of the long penult as making that syllable prominent, without any stress on the one hand or any elevation of pitch on the other? And in words like *lātuit*, *hōmīnes*, etc., may not the rule that these words were accented on the antepenult simply mean that, in consequence of the short penult, that syllable did *not* possess any prominence, and hence, after the establishment in Latin of the three-syllable law, the syllable next preceding became the conspicuous one?

There are, of course, objections that at once suggest themselves against so radical a view as the foregoing. I do not pretend to have established it, but simply to have suggested a theory of Latin accent which all must admit is possible and which to me seems even probable. At all events, it is certainly of the first importance in approaching so delicate a problem as the pronunciation of a language whose data we can no longer fully control, first to rid ourselves as completely as possible of all preconceived notions derived from our own language which might mislead us, and to take into account the great divergence of human speech

<sup>1</sup> Cf. Seelmann, *Die Aussprache des Latein*, p. 16, 2, Accentarten.

along with the often radically different character of spoken languages.

The thesis, then, that classical Latin was absolutely unstressed may not be proven; but that, if stressed, classical Latin was only slightly so, was, I think, made fairly certain by the argument advanced at the outset of this paper<sup>1</sup>; and it is precisely this slightly stressed (possibly absolutely unstressed) character of the Latin language that explains the character of Latin poetry. Stress was either absent or at most quite subordinate; hence syllabic quantity came to be the natural basis of verse. Theo-

<sup>1</sup> This seems to me all but universally admitted among those who hold to the stress view of Latin accent. Cf. Ritschl, *Prolegomena ad Trinummum*<sup>1</sup>, p. 207; Madvig, *Latin Grammar*, §498, note; W. Meyer, *Ueber die Beachtung des Wortaccentes in der altlateinischen Poesie*, p. 5 f.; Lucian Müller, *De re metrica poetarum Latinorum*<sup>2</sup>, p. 233. This conclusion rests not only upon the internal evidence above adduced, but also upon the statements of Roman writers of the best period. Cicero and Quintilian, in discussing the question of metrical sequences in prose, particularly at the close of a sentence, nowhere pay the least attention to word-accent, but do lay special stress upon quantitative distinctions. Cf., e. g., Cicero, *Orator*, 55, 56 (§§183-90), 64 (§§215-18); Quint., *Inst. Or.* IX 4, 47; 61; et passim. From the definitions of accent given by the ancient grammarians, it is impossible to gather any consistent conception of the phenomenon. See the *testimonia* gathered by Schöll in his *De accentu linguae Latinae*, p. 73 ff. Of the formal definitions, none antedates the fourth century, and many are much later. Most of these, moreover, are extremely vague. Dositheus, for example (Keil, *Gram. Lat.* VII, p. 377, 6), defines accent as "unius cuiusque syllabae proprius sonus." Maximus Victorinus (Keil, VI 188, 15) and Audax (VII 322, 12) as "unius cuiusque syllabae in sono pronuntiandi qualitas." This same conception of the *qualitas syllabarum* appears also in Sergius, *Explan.* (Keil, IV 528, 28). Ps.-Priscian (III 519, 25) is more precise. According to him, "accentus namque est certa lex et regula ad elevandam et deprimendam syllabam unius cuiusque particulae orationis." The *Codex Bernensis* 16 (K. Sup. XLV) gives "accentus est vox syllabae, quae in sermone plus sonat de ('than') ceteris syllabis." Similarly Servius (K. IV 426, 16) and Pompeius (K. V 126, 10). Three grammarians define accent as *anima vocis*, viz. Diomedes (K. I 430, 30), Pompeius (K. V 126, 27), and the *Cod. Bern.* 16 (K. Sup. XLV). Lastly Diomedes (K. I 430, 29) says: "accentus est acutus vel gravis vel inflexa elatio orationis vocisve intentio vel inclinatio acuto aut inflexo sono regens verba." From this chaos one can hardly have the conscience to seek support for a theory. The testimony of Diomedes might be cited in support of a stress accent, just as that of Ps.-Priscian might be cited in support of a musical accent. But what with the lateness of these writers, their mutual contradictions, and the suspicion that some of them at least are but echoing statements made by Greek grammarians concerning the Greek language, it is safest to base no positive view of the nature of accent upon their utterances.

retically, now, this quantitative Latin poetry may seem sufficiently simple, were it not for the so-called *ictus*, a feature to which our traditional prosody uniformly gives a prominent place. What was this *ictus*? It is usually defined as stress accent. With a single exception to be noted below, it is invariably thus defined, so far as I am aware. Yet I question whether there is a particle of legitimate evidence, internal or external, in support of this view. The conception of *ictus* as stress accent seems to me to have its foundation solely in the practical assumption that Latin poetry was, like English and German poetry, really accentual.<sup>1</sup> I say 'practical assumption.' It would, of course, be absurd to maintain for a moment that theoretically the quantitative character of Latin verse has ever been denied. Yet so long as Latin is pronounced with absolute disregard of vowel quantity, as it necessarily is by the so-called English method of pronunciation, and as it habitually is in Germany to my certain knowledge, or with disregard of syllabic quantity, as it usually is even where the Roman pronunciation is nominally followed, so long is it inevitable that any theoretical recognition of the truly quantitative character of Latin verse should be totally clouded by the impulse toward securing a rhythmical effect. By a pronunciation which yields *gērō*, *tērō*, *ingēniūm* and thousands more of the same sort, on the one hand, and *filius*, *his*, *vis*, etc., on the other, a quantitative verse is as impossible as would be an accentual verse in English, were we to misplace the regular word-accent. It is no exaggeration to say that were we to accent Longfellow's line as follows:

This is the forést primeval, the murmuríng pines,

the result would be no whit worse than is inevitably necessary by any system of Latin pronunciation which fails scrupulously to observe the quantity of every vowel and of every syllable. A neglect of quantity was inevitable under the English pronunciation of Latin; it is inevitable under the pronunciation of Latin current in Germany. Neglect of quantity leaves nothing except accent as a basis for a metrical effect, and naturally leads to an accentual reading of Latin verse, which brings with it the conception of *ictus* as a stressed syllable. Yet this conception seems to me demonstrably false, for the following reasons:

<sup>1</sup>Cf., for example, Christ, *Metrik der Griechen und Römer*<sup>2</sup>, p. 3, where the strongly stressed character of German utterance is appealed to in support of the stress theory of *ictus* for the classical languages.

1. So far as we know, no language is ever forced to an artificial pronunciation when adapted to the service of poetry. It is irrational to conceive any such adaptation.<sup>1</sup> The poet simply takes the choicer words of familiar speech and employs them in their ordinary equivalence with their regular pronunciation. He must do so. For his appeal is to the many, not to a select handful who may have been initiated into the secret trick of his versification; hence he must use words in the pronunciation familiar to his auditors or readers. Otherwise he can make no appeal. His art consists, on the mechanical side at least, in arranging words in such a way that the poetic form is obvious to the meanest observer who knows the words by ear or eye. Can any poetry be cited in any language of which this is not true? Is it then not absurd to assume that in Latin poetic form consisted in employing words with gratuitous stress accents unknown in the ordinary speech? Can we conceive of an *atavis*, a *regibis*, a *Troiaé*, a *canó*, or a thousand other equally grotesque hermaphrodites that we are compelled to father by this theory? And is it credible that poetry so inconceivably artificial should have been tolerated, not to say admired, by such sober-minded persons as the Romans?

2. The view that *ictus* was stress is to be rejected because it involves the assumption of a second basis for Latin verse. We have already noted that Latin verse is quantitative, i. e. a dactyl is a long time followed by two short times. But if *ictus* is stress, then a dactyl is a stressed syllable followed by two unstressed syllables. We should thus get two principles as the basis of Latin verse, quantity and accent (i. e. stress), and it seems to me impossible that there should uniformly and regularly have been two principles at the basis of Latin verse or any other.

3. It is nowhere hinted or implied in the ancient writers that *ictus* was stress. To judge from the prominence assigned to *ictus*

<sup>1</sup>Cf. Schöll, *De accentu linguae Latinae*, p. 25, note: *nihil poeta inferit in linguam, sed quae praebet elementa disciplina et humanitate excolit, perpolit, adauget.* This principle would not merely appear to be a rational necessity, but is supported by distinct testimony from the best period. Cf. Cic. *de Orat.* III 45, 177, where it is clearly asserted that the elements of prose and verse are identical; also *Orator*, 11, 36, where Ennius is praised for adhering to the common speech in his poetry (*communis mos verborum*). Yet Corssen, *Aussprache, Vok. u. Betonung*, II<sup>2</sup>, p. 975, maintains that just such an artificial adaptation in pronunciation did occur in Latin verse. Similarly Lucian Müller in both editions of his *De re metrica* (2d ed., p. 234). The same view also is implied wherever *ictus* is defined as stress.

in our grammars and other works on prosody, one might expect to find that the word was widely current as a technical term among the ancients. Such, however, is not the case. Among all the systematic discussions of prosody found in the Latin grammarians I have been able to discover no definition of the term,—in fact no mention of it as a technical term of prosody.<sup>1</sup> The word does occur a few times in the classical period, but so rarely and in such context that there is no justification for regarding it as a *terminus technicus*. Thus we find it in the familiar passage of Horace, ad Pisones, 253:

unde etiam trimetris accrescere iussit  
Nomen iambeis, cum senos redderet ictus  
Primus ad extremum similis sibi.

More frequently we find *ictus* in this signification combined with *digitus*, *pollex*, or *pes*. Thus Horace, Carm. IV 6, 36 *pollicis ictum*; Quint., Inst. Or. IX 4, 51 *pedum et digitorum ictu intervalla signant*; Pliny, N. H. II 95, 96, 209 *ad ictum modulantium pedum*. From these and similar instances (the total number, however, is very small), the natural inference is that *ictus* as a metrical term primarily designated taps of the feet or fingers, and was then transferred to denote the rhythmical beats of verse. Certainly there is no evidence either from the etymology of the word or from its use in any citable case to indicate that it designated vocal stress.

Scarcely more support of the stress theory can be derived from the use of the words *arsis* and *thesis* as employed in the systematic treatises on Latin prosody prepared by the ancient grammarians. It is commonly held that the grammarians and metricians used these designations with an inversion of the application which obtained in Greek. But as Westphal has pointed out (Griechische Rhythmis, p. 106), such is by no means the case; the Roman writers exhibit no little confusion in this matter, it is true, but they rarely use *arsis* in the sense of Greek *θέσις*. Probably Marius Victorinus is the only metrician<sup>2</sup> who does this,<sup>3</sup>

<sup>1</sup> Professor Hale (Proceedings Amer. Phil. Assoc. 1895, vol. XXVI, p. xxx) implies that the word *ictus* is employed as a technical metrical term by Charisius. His view and the argument based upon it will be discussed later in this paper.

<sup>2</sup> On Ps.-Priscian, *De accentibus* (Keil, III 521, 24), see below.

<sup>3</sup> Cf. Westphal, l. c., p. 107 f.; Weil et Benloew, *Théorie générale de l'accentuation latine*, p. 98 f.

and he does it but once (Keil, VI 40, 16 f.). The actual usage of the Romans in employing the terms *arsis* and *thesis* can be seen only by an examination of the decisive passages in which these words occur. The commonest definition explains *arsis* as *elevatio* (*sublatio*), *thesis* as *positio*. This bare statement, without the addition of any explanatory remarks, is found in Sergius, *Explanat.* in *Don.* I (Keil, IV 523, 2); Cledonius (Keil, V 30, 10); Atilius Fortunatianus, *Ars* (Keil, VI 281, 4). Similar is Terentianus Maurus's explanation, *de Metris*, 1345 (K. VI 366):

parte nam attollit (*sc. pes*) sonorem, parte reliqua deprimit.

Other writers add *vocis* as explanatory of *elevatio* and *positio*. Thus, Martianus Capella, IX (365, 17 Eyssenhardt): <*arsis est elevatio*>, *thesis depositio vocis ac remissio*; Isidore, Orig. I 16, 21 *arsis et thesis, id est vocis elevatio et positio*; *Commentum Einsidense in Don. Artem Mai.* (Keil, Suppl. 228, 23): *arsis elevatio sc. vocis, eo quod ibi vox elevetur. Thesis humiliatio vel demissio quia ibi vox deponatur.*

A new conception appears in the three following writers:

Juliani Excerpta (Keil, V 321, 12): *quid est arsis? Elevatio, id est inchoatio partis. quid est thesis? positio, id est finis partis* . . . In trisyllabis, si in prima habuerit accentum, ut puta dominus, duas syllabas vindicat *arsis et unam thesis. si paenultimo loco habuerit accentum, ut puta beatus, arsis vindicat unam syllabam et thesis duas.*

Servius in *Donatum* (Keil, IV 425, 7): *Arsis dicitur elevatio, thesis positio. quotienscumque contingit ut tres sint syllabae in pede . . . si in prima syllaba fuerit accentus, arsis duas syllabas possidebit; si autem in media syllaba, thesi duas syllabas damus.*

Pompeius Comm. in *Donati artem* (Keil, V 120, 29): *arsis et thesis dicitur elevatio et positio . . . Romulus quando dicimus, prima syllaba habet accentum: dicimus duo in arsi, unum in thesi . . . si media syllaba accentum habuerit, ultimae syllabae iungis plura tempora, ut arsis habeat unum, thesis duo.*

Marius Victorinus, Art. Gram. I (Keil, VI 40, 14), evidently impelled by the spirit of Goethe's maxim: "Besonders lass genug geschehen," gives us a unique ragout:

*Arsis igitur ac thesis quas Graeci dicunt, id est, sublatio et positio, significant motum pedis. est enim arsis sublatio pedis sine sono [sc. pedis],<sup>1</sup> thesis positio pedis cum sono (the Greek*

<sup>1</sup> Westphal, Gr. Rhythmik, p. 105.

conception): item arsis elatio temporis, soni, vocis: thesis depositio et quaedam contractio syllabarum . . . in dactylo vero tollitur una longa, ponuntur duae breves (the reverse of the Greek conception)<sup>1</sup>; while elsewhere (Keil, VI 45, 2) the same writer evidently regards the arsis as the *first syllable* of the foot without regard to quantity. The passage reads: horum [the trochee and iambus] arsis et thesis alterna mutatione variatur, si quidem in iambo arsis primam brevem, in trochaeo autem longam habeat *incipientem*, thesis vero contraria superioribus sumat. This conception has already appeared in the passage of Julianus above cited (Keil, V 321), and appears again in Sergius (Keil, IV 480, 13): scire etiam debemus quod unicuique pedi accidit, arsis et thesis, hoc est elevatio et positio; sed arsis in prima parte, thesis in secunda ponenda est; Diomedes (Keil, I 480, 10): iambi enim arsis unum tempus tam in se habet et eius thesis duo quam trochaei versa vice arsis duo habet et thesis unum; Terentianus Maurus, 1388 (Keil, VI 367):

*ἀρσις* unum possidebit, quando iambum partior; fiat alternum necesse est, cum trochaeum divides.

It remains only to cite the testimony of Ps.-Priscian (Keil, III 521, 24): nam in unaquaque parte orationis arsis et thesis sunt, non in ordine syllabarum sed in pronuntiatione: velut in hac parte; natura. quando dico natu, elevatur vox, et est arsis intus. quando vero sequitur ra, vox deponitur, et est thesis deforis. quantum autem suspenditur vox per arsin, tantum deprimitur per thesin. sed ipsa vox, quae per dictiones formatur, donec accentus perficiatur, in arsin deputatur; quae autem post accentum sequitur, in thesin.

The foregoing are the essential specific expressions of opinion on this subject which I have discovered among the ancient Roman writers on metric. It has seemed worth while to cite their statements in full in order that we may see exactly what support they furnish for the traditional theory of ictus and arsis. In my judgment they afford no confirmation whatever of the view that ictus in the classical period was stress. In the multitude of testimonies which I have cited it is impossible to find any definite, coherent common doctrine. The witnesses not merely contradict one another; many of them, as we have seen, contradict themselves, till one involuntarily exclaims with Weil and

Benloew: "Rien n'est plus difficile à expliquer qu'un auteur qui ne sait pas lui même ce qu'il veut dire."<sup>1</sup> Of the writers above cited it seems most natural to believe that those who define arsis and thesis as *sublatio* and *positio* are merely translating the terms of the Greek writers without any serious attempt to understand their actual application. Those who add to this definition the statement that the arsis was the initial syllable of the foot, the thesis the last part, are apparently guilty of attempting to combine two irreconcilable conceptions. Both of these, strange to say, are found among Greek writers. The late Greek metricians applied the term *ἀποις* indiscriminately to the initial syllable of the foot, quite irrespective of its quantity.<sup>2</sup>

As regards those writers who define arsis as *elevatio vocis*, the earliest of these is Martianus Capella, who does not antedate 400. Even conceding that *elevatio vocis* could by any possibility have been deliberately intended to mean 'stress of voice,' we have to bear in mind that a century and a half before the time of Martianus Capella quantitative Latin poetry had begun to be supplanted by accentual poetry. Commodianus is usually cited as the first versifier who exemplifies the transition.<sup>3</sup> He wrote about 250. It is an interesting fact also that Martianus Capella himself, in those passages where he essays poetic form, repeatedly yields to the spirit of the age and employs an accented short syllable where the metre demands a long one.<sup>4</sup> If, therefore, Martianus Capella, Isidore, and the author of the *Commentum Einsidlense* really meant stress by *elevatio vocis*, the presumption is strong that their testimony holds only for the accentual poetry of their own day, not for the quantitative verse of the classical period.

Another difficulty confronts us. How are we to reconcile the statements of Julianus (Keil, V 321, 12), Servius (Keil, IV 425, 7), and Pompeius (Keil, V 120, 29) with the theory of an accented arsis? These writers tell us that in words of the type of *Romulus*, *dominus*, the arsis consists of two syllables. Certainly a stress accent cannot stand simultaneously upon two successive syllables. Probably any attempt to reconcile these last three statements with those previously cited would prove futile. Julianus, Pompeius, and Servius are apparently concerned with the phenomena of individual words rather than with metrical feet. With them

<sup>1</sup> *Théorie générale*, p. 100.

<sup>2</sup> Westphal, *Gr. Rhythmik*, p. 106 f.

<sup>3</sup> See Teuffel-Schwabe, *Gesch. d. röm. Lit.*, §384.

<sup>4</sup> Teuffel-Schwabe, *ibid.*, §452, 5.

arsis manifestly has some connection with the accented syllable, though it is clearly more than that. A similar attitude appears in Ps.-Priscian, *De accentibus* (Keil, III 521, 24 f.), where all of a word preceding the accented syllable (and including that) is reckoned as belonging to the arsis, the remainder to the thesis.<sup>1</sup>

I have been thus minute in considering in detail the testimonies of the Roman metricians, because it seemed to me that we could in no way see so well how completely they fail to afford the slightest support to the stress theory of Latin ictus or arsis.<sup>2</sup>

I have already given three reasons why it seems to me erroneous to regard ictus as stress: 1. Because it involves the importation of a stupendous artificiality into the reading of verse. 2. Because it involves a dual basis for versification,—stress as well as quantity. 3. Because the view finds no support in any ancient testimony. To these three reasons I wish to add as 4. There are excellent grounds for believing that ictus was something else than stress. If Latin poetry was quantitative, as its internal structure and all external evidence seem to show, then a dactyl was a long time followed by two short times, and a trochee a long time followed by one short time, absolutely without any other parasitic accretion. When, now, we come to use dactyls by the line, one part of every foot will inevitably be felt as prominent, viz. the long syllable. The relative amount of time given the long syllable of every dactyl naturally brings that long syllable into consciousness, and especially must it have done so to the minds of the Romans, whose nice quantitative sense is proved by the very fact that they made quantity the basis of their versification. Yet the long of the dactyl has no stress.<sup>3</sup> It is natural for us to stress it, us whose only conception of verse is accented verse. But in so doing I believe we are simply transferring to Latin verse our own inherited verse-sense. I define ictus, therefore, not as stress, nor as accent, but simply as the

<sup>1</sup> Julianus (Keil, V 321) reckoned only the accented syllable of a trisyllable as belonging to the arsis. The preceding and following syllables he reckoned with the thesis.

<sup>2</sup> Sergius (Keil, IV 483, 14), cited by Christ (*Metrik*<sup>2</sup>, p. 59) as supporting the view that arsis was stress, ought not to be quoted in defence of this view. Sergius is not here speaking of metrical feet, but merely of individual words that accidentally form feet. The context makes this perfectly clear.

<sup>3</sup> Of course it may incidentally have stress, if the word-accent fall on this syllable. But this stress was at most relatively slight, as already explained. On the rôle played by word-accent in reading verse, see below.

quantitative prominence inherent in a long syllable. This definition applies primarily only to the four fundamental feet—the dactyl, the anapaest, the trochee, and the iambus. It does not apply to the spondee, for example, when substituted for the dactyl in dactylic verse. In such cases the *first* long of the spondee is felt as the quantitatively prominent thing in the foot, even though the second syllable of the spondee is also long. In dactylic verse, the dactylic character and feeling so dominate the line that any spondee naturally takes on a dactylic character and is felt to be quantitatively prominent in its *first* syllable, just as in the case of the dactyl itself. So in iambic measures, where the tribrah or dactyl is substituted for the iambus, the quantitative prominence inherent in the long syllable of the iambus is felt as transferred to the two final shorts of the tribrah or the dactyl.<sup>1</sup>

This conception of thesis or ictus receives no little support from the positive testimonies of the Roman grammarians. These writers in their definitions of arsis and thesis repeatedly call attention in unambiguous phrase to the essentially quantitative character of these concepts. In this, their agreement is conspicuous. Thus:

Diomedes (Keil, I 474, 30): *pes est sublatio ac positio duarum aut trium ampliusve syllabarum spatio comprehensa. pes est poeticae dictionis duarum ampliusve syllabarum cum certa temporum observatione modus recipiens arsin et thesin;*

Marius Victorinus (Keil, VI 41, 25): *nam rhythmus est pedum temporumque iunctura velox divisa in arsin et thesin vel tempus quo syllabas metimur; id. VI 43, 26: signa quaedam accentuum ... syllabis ad declaranda temporum spatia superponuntur ... sed et hoc non praetermiseric, eosdem [Graecos] figuras pedum secundum spatia temporum per litteras ita designasse, ut brevis syllabae loco, quae sit unius temporis, ponatur B [βραχν], longae autem loco, quae sit temporum duum, M [μακρόν]: hoc ideo, ut per litteras regula pedum facile intellegatur.*

Atilius Fortunatianus (Keil, VI 281, 4) speaks of feet as things “qui gressibus alternatis quasi incedunt per versus et moventur”;

Commentum Einsidlense (Keil, Suppl. 228, 9): *his [sc. pedibus] ... ad peragendos versus tempora syllabasque metimur ... nulla enim alia res dinumerat tempora et syllabas velut pedes ...*

<sup>1</sup> On the baselessness of the traditional pointing of dactyls and tribrahcs in iambic measures (— $\circ$   $\circ$ ,  $\circ$   $\circ$   $\circ$ ), see the latter part of this paper.

*pes est certa dinumeratio syllabarum vel certa dinumeratio temporum.*

All of these definitions and observations exhibit a striking unanimity in emphasizing the purely quantitative character of ancient poetry.<sup>1</sup> Even when signs were employed by the Greeks to mark the syllables, these signs were abbreviations of words for 'long' and 'short,' not for 'stressed' and 'unstressed.'<sup>2</sup>

To those who may cherish a scepticism as to the tangible reality of 'quantitative prominence,' I would only say that that phrase need appear shadowy to no one who will actually read one thousand lines of Latin aloud *with absolute fidelity to vocalic and syllabic quantity*. My own revolt against the traditional view of ictus has been purely and solely empirical. It was simply because by faithful practice in accurate reading my ear quickly grew sensitive to quantitative differences, that I was forced to believe that, as quantity was the basis of Latin verse, so ictus was only quantitative prominence. This conclusion, I say, was first forced upon me empirically, and the theoretical formulation was entirely subsequent to, and solely the result of, my actual oral experience in reading Latin. No one, in my judgment, can approach this subject in a candid spirit who has not first taken the pains to acquire the habit of exact pronunciation of Latin vowels and syllables. Even in this country, where we have nominally adopted the quantitative pronunciation of Latin, we have still much to learn in this matter. Our shortcomings are so pronounced, and bear so directly upon the theoretical aspect of the question at issue, that I shall here venture to recapitulate some of them.

First, we habitually neglect vowel quantity. One cause of this is the vehement stress which (in accordance with our English-speaking instinct) we regularly put upon the accented syllable. The word *ēvitābātur*, for example, contains four successive long vowels. Yet in ninety-nine cases out of a hundred, the penultimate syllable is so strongly stressed that the first three vowels are pronounced short. In Latin poetry the result of such pronun-

<sup>1</sup> Cf. Aristoxenus's designation of ἀρσις and θέσις as χρόνοι, χρόνοι ῥυθμικοί, χρόνος ποδικός, Westphal, Gr. Rhythmic, p. 103; also as ὁ ἀνω χρόνος, ὁ κάτω χρόνος, ibid., p. 104.

<sup>2</sup> The modern practice of using the acute accent to designate the thesis—a practice which is in itself an assumption of the stress view of ictus and has done so much to propagate it—goes back only to Bentley.

ciation is to wreck the quantitative character of the verse as effectively as if in English we were to misplace the accents on successive syllables. How much poetic form would appear in Milton's opening line of *Paradise Lost*, were we to pronounce 'Óf man's first disóbedfence,' for instance? Besides destroying vowel quantity as a result of over-stressing the accented syllable, we habitually neglect it in hundreds of other instances where there is no such disturbing factor. By some strange fatality the *-is* of the genitive singular is commonly pronounced *-ɪs*, while the *-is* of the ablative plural as regularly is heard as *-ɪs*; while the number of such pronunciations as *pāter, āger, nīsī, quōd, quībus, ingēniūm* is simply legion. No one who pronounces Latin in that way can expect to feel the quantitative character of a Latin verse, and is in no proper frame of mind to give the quantitative theory dispassionate consideration; for one or two false quantities destroy as completely the quantitative character of a verse of Latin poetry as would one or two misplaced accents any English verse.

Even more serious than our neglect of vowel quantity is our neglect of syllabic quantity. The shipwreck resulting from neglect of vowel quantity occurs chiefly in *open* syllables, i. e. in syllables whose vowel is followed by a single consonant, which always belongs to the following vowel, thus leaving the preceding syllable open. In such syllables the quantity of the vowel is always identical with the quantity of the syllable; so that a false vowel quantity involves the quantity of the syllable as well. In closed syllables, on the other hand (i. e. syllables ending in a consonant), an error in vowel quantity does not affect the quantity of the syllable. I may pronounce *vēndō* or *vēndō*. In either case the syllable will be long.<sup>1</sup> Hence in closed syllables an

<sup>1</sup> All closed syllables are phonetically long. This is a principle universally accepted by the phoneticians. Yet Professor Hale in *Harvard Studies*, VII, p. 267, n., contests it. I can only refer to such standard works as Sievers, *Grundzüge der Phonetik*<sup>2</sup>, §35, 1: "In Wirklichkeit können nur solche Silben für kurz gelten, welche auf einen kurzen Sonanten (= vowel) ausgehen, also solche wie *ra, la, pra, fra*, etc. Alle geschlossenen Silben aber sind lang." In the following note Sievers adds: "Die übliche Definition der positionslangen Silben, spricht allerdings von mehr als einem Consonanten hinter dem Sonanten (= vowel); in Wirklichkeit aber genügt der Ausgang der Silbe auf einen Consonanten um sie lang zu machen." Professor Hale offers no argument against this current doctrine of the phoneticians. The chief objection he adduces is that the initial syllable is short in English *many* and *battle*. But in each case the syllable is open (*mă-ny, bă-tl*). Cf. also Havet in *Mémoires de la Société de linguistique*, IV 22 f., who points out that it is not the two

error in vowel quantity does not destroy the quantity of the syllable, and so does not interrupt the quantitative character of a Latin verse. But the syllable must be actually closed in pronunciation; *else where the vowel is short, the syllable will be left open, and will be metrically short*, destroying the verse. It is precisely here that we err so frequently and so fatally in our reading of Latin verse. We do not close the syllables that ought to be closed and were closed by the Romans. The commonest class of words where we commit this error are those containing a geminated consonant—words of the type of *ges-serunt*, *ac-cipio*, *at-tigerat*, *ter-rarum*, *ap-parabat*, *an-norum*, *ad-diderat*, *flammarum*, *excel-lentia*, *ag-gerimus*. These words we habitually pronounce in prose and verse alike, as *gē-serunt*, *ā-cipio*, *ā-tige-rat*, *tē-rarum*, *ā-parabat*, *ā-norum*, *ā-didit*, *flā-meus*, *excē-lentia*, *ā-gerimus*. Words of this type are extremely frequent in Latin. I have counted forty-five in the first hundred lines of Virgil's Aeneid, i. e. the pronunciation described destroys the quantitative character of the Latin verse at forty-five distinct points, often twice in the same verse.

Nor is this all. In other combinations in the interior of words we are often guilty of quite as serious errors. In English, besides

consonants which lengthen the syllable, but that the syllable is lengthened because one consonant is joined to the previous vowel, while the other consonant is joined to the following vowel. Professor Hale (l. c.) advances the view that in *iste*, for example, the first syllable is long because the time of the consonant *s* adds to the vowel (= one mora) an equal amount of time. But I feel confident that Havet is correct when he says (l. c., foot-note, p. 24): "It is not the *s* which takes time. It is the silence between the *s* and the *t*. The duration of the consonants themselves is, if not nil, at least a negligible quantity." This may be clearly seen at the beginning of words. For example, the first syllable of *scribās*, despite its three consonants, takes appreciably no longer time than the first syllable of *vivas*. What really makes the long quantity is the closed syllable with its accompanying pause, not the 'obstructed consonant,' as Professor Hale would hold. A good English example of how the closed syllable may be long even when *no* consonant follows, may be seen in the phrase *at all*. Ordinarily we pronounce this as one word, *ā-tl(l)*; but occasionally it is pronounced as two words, *āt al(l)*, the first of which is closed and is phonetically long. Our English *a tall* (for *at all*) explains too the short quantity of final syllables ending in a consonant before a word beginning with a vowel. Such syllables are not actually closed, but the final consonant combines by a natural *liaison* with the following vowel, e. g. *ama talias* (= *amat alias*). See Sievers, *Grundzüge der Phonetik*<sup>4</sup>, §658, who cites *τὸν αὐτὸν*, "gesprochen *to-nay-ton*." At the end of a verse, however, such syllables are actually long.

rather, *gē-*  
*ād-idit*  
*flā-meus*  
putting the conso-  
nant with the  
stressed syllable

7	3	2	3	2	3	2	3
3	2	3	2	3	2	3	2
2	3	2	3	2	3	2	3
3	2	3	2	3	2	3	2
2	3	2	3	2	3	2	3

*muta cum liquida*, there are many other consonant combinations with which in stressed syllables we show a regular tendency to begin the syllable. This is especially true of the combinations *sp*, *sc(k)*, *st*, *squ*; also *scl*, *scr*, *str*. This tendency of our vernacular speech naturally affects our pronunciation of Latin words in which these combinations occur. The *s* of such combinations properly belongs with the preceding vowel, in order that the preceding syllable may be closed and so made phonetically long; yet we frequently (almost invariably, according to my observation) join the *s* with the consonants of the tonic syllable. I refer to such pronunciations as *ä-spér(r)ima*, *i-stiūs*,<sup>1</sup> *tempē-stáribus*, *corū-scábat*, *mi-sciéral*, *magi-strórum*, *ä-sclépias*, *ä-scripsit*, *qui-squíliae*.<sup>2</sup> My own students often exhibit a tendency to combine *ct*, *pt*, *ps* with a following accented vowel, and produce short syllables in such words as *volū-ptáte*,<sup>3</sup> *ä-spē-clórum*, *i-psius*. Where the accent rests on the vowel immediately preceding these combinations, the liability to error is very slight.

There are yet other cases in which error is frequent, if not habitual. Unstressed syllables whose vowel is followed by *r* + any consonant are particularly liable to be made phonetically short in those portions of the country where the *r* is neglected. This is especially true in the eastern part of the United States, where *pō-(r)tárum*, *tē-(r)minorum*, etc., represent the prevailing utterance.<sup>4</sup> The combination of *m* or *n* also with a following explosive in unstressed syllables frequently is so treated as to shorten syllables phonetically long. The process by which this is accomplished is not yet clear to me. Observation, however, has taught me that in such words as *imperator*, *intendo* the first

<sup>1</sup> In Early Latin this division was probably common. I should so explain the metrical use of the word by Plautus and Terence. Cf. Humphreys, *Proceedings of the American Phil. Assoc.* 1895, vol. XXVI, p. xxxi.

<sup>2</sup> Lest our traditional rules for syllabication be cited in support of the division here criticized, I would say that the traditional rules, though laid down by the ancient grammarians, can hardly have been more than practical working directions for copyists and stone-cutters. It is impossible that they indicate the actual phonetic division of the syllables. See Appendix to my Latin Grammar, p. 31 f. Since the publication of the Appendix, Professor Hale, in *Harvard Studies*, VII, p. 249 f., while expressing dissent from certain slight details of my arguments as stated in the Appendix, has endorsed the main proposition there laid down and has fortified it by additional data.

<sup>3</sup> So probably in Early Latin. Cf. note 1, above.

<sup>4</sup> In stressed syllables, where the *r* is neglected, the vowel is regularly lengthened, e. g. *pō-ta*.

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syllable is frequently made short; whether by omission of the nasal, by pronouncing a short nasalized vowel, or by a short *nasalis sonans* (ñ), I do not undertake to say. The fact, I believe, is beyond question.

There is only one other class of cases to which I shall call attention, viz. the unconscious *liaison* of final *s* after a short vowel with the initial consonant of the following word. Where the following word begins with *s, p, c, t, v, m, n, f*, etc., and where the connection of sense is close, this *liaison* is in my experience frequent. It is not surprising that it should be, for we habitually join a final *s* of an unstressed syllable<sup>1</sup> in our own speech with a following *s, c, t*. Examples in Latin are: *urbi sporta, capi scannem, urbi svici*.<sup>2</sup> A case that puzzled me for a time was Juv. III 53 *carus erit Verri*, as read by a student. The fourth syllable sounded short to my ear, and it was only after repeated readings that I discovered that the reader was really dividing: *carus erit Verri*.<sup>3</sup> I do not say that this *liaison* is invariable. It is certainly frequent, and, where it occurs, must vitiate the quantitative effect of the verse.<sup>3</sup>

These common errors in reading Latin must be clearly understood, if they are to be remedied. It is by no means an extremely difficult matter to acquire an exact quantitative pronunciation. It takes time and pains and considerable oral practice. I do not believe that it requires a particularly sensitive ear. By practice in rigidly exact reading, the quantitative sense is not slow in coming; but without that exactness it cannot come and cannot be expected to come. He who has once developed the quantitative sense will, I am confident, feel no need of any artificial stress.

The foregoing views as to the nature of ictus had long been matured and had been presented to class after class of college students when I stumbled on the following neglected remark of Madvig (Latin Grammar, §498, N.): "We should also guard against the opinion which is generally current; viz., that the ancients accentuated the long syllable (in the arsis) and distinguished in this way the movement of the verse (by a so-called

<sup>1</sup> In Latin the final syllable, of course, is regularly unstressed.

<sup>2</sup> *Sv*, i. e. *sw*, is a sufficiently common initial combination in English; the same is true of *tv*, i. e. *tw*.

<sup>3</sup> It may be a question whether the so-called weak pronunciation of final *s* in Early Latin is not, after all, merely a phenomenon of *liaison*, the *s* going with the following consonant, e. g. in Ennius's *plēnū sfidēt* and *miserrimū snuntiu smortis*.

verse-accent, *ictus metricus*), and consequently often accentuated the words in verse quite otherwise than in prose (e. g. *Arma virumque canó Trojaé qui primus ab oris; Italiám fató profugús Lavinaque venit*), which is impossible; for the verse depends on a certain prescribed order and form of movement being distinguishable, when the words are *correctly*<sup>1</sup> pronounced. In our own verses we do not accentuate the syllables *for the sake of the verse*, but the syllables which are perceptibly distinguished by the accentuation in prose *form verse* by being arranged to succeed each other in this way. In Latin and Greek (where even in prose pronunciation the accent was quite subordinate, and is never named in speaking of rhetorical euphony, while on the other hand the difference of quantity was distinctly and strongly marked), the verse was *audibly distinguished* by this very alternation of the long and short syllables." So far my assent with Madvig is complete. He goes on: "But as it is not possible for us either in prose or in verse, to pronounce the words according to the quantity *in such a way* as the ancients did, we cannot recite their poetry correctly, but are forced in the delivery to give a certain stress of voice to the arsis, and thus make their verses somewhat resemble ours. It should, however, be understood, that it was different with the ancients themselves (until the last century of their history, when the pronunciation itself underwent modifications)." These words of Madvig were written in 1847—over half a century ago. At that time it is not strange that he should have denied the possibility of our reading Latin verse quantitatively with substantial accuracy. Even before the end of his life, it is likely that Madvig relinquished this part of his earlier opinion.

As regards word-accent in the reading of Latin verse, I believe that it retained its full value; for as I have maintained that in poetry words are used with their ordinary prose values, and are pronounced without addition of foreign elements, so I believe that they were pronounced without subtraction of any of their elements.<sup>2</sup> Herein I agree entirely with Professor Hale (Proceedings Am. Phil. Assoc., vol. XXVI, p. xxvii).<sup>3</sup> But we have

<sup>1</sup> The italics are Madvig's.

<sup>2</sup> In support of this we have also the clear testimony of the ancients. See the abundant references in Christ, Metrik<sup>2</sup>, p. 59.

<sup>3</sup> I regret, however, that this scholar is not as consistent in refusing to admit into verse what was *not* in prose (artificial stress), as he is in refusing to relinquish what *was* in prose (word-accent).

already seen that the Latin accent was slight. It was precisely that fact which led the Romans of the classical period to make quantity the basis of their verse. Assuming, now, that the word-accent was very slight, and possibly was even merely quantity or absence of quantity in penults, what wonder that, with quantity predominant in the verse and *in the Roman consciousness*, such slight word-accent as existed was felt as no intrusion? An analogous situation reveals itself in our English verse. Our verse is primarily accentual, and yet each syllable has its quantity, and shorts and longs mingle harmlessly with accented and unaccented syllables. Why should not the reverse have occurred in Latin just as simply and just as naturally?

To sum up, then: Latin poetry is to be read exactly like Latin prose.<sup>1</sup> Latin was primarily a quantitative language in the classical period and is to be read quantitatively. The Latin word-accent was relatively slight as compared with that of our strongly stressed English speech, and is therefore to be carefully subordinated to quantity both in prose and poetry. Ictus was not a metrical term current among the Romans, nor was there anything corresponding to it in the quantitative poetry of the Greeks. The term is purely modern. We first imported the conception of stress from our modern speech into the quantitative poetry of the Greeks and Romans, and then imported the term ictus to cover it. But just as the conception of artificial stress in Latin poetry is false, so the term ictus is superfluous.<sup>2</sup> *Θέσις* was employed by the ancient Greek writers on metric to designate the prominent part of every fundamental foot, and is still entirely adequate to cover that conception.

It remains only, in conclusion, to meet certain criticisms which have been made upon my conception of ictus (thesis). In the Proceedings of the American Philological Assoc. XXVI, p. xxx, Professor Hale has characterized my view as immature and has advanced certain objections against it. These objections are four in number:—

<sup>1</sup>I forbear to enter into any discussion of the difficult matter of elision of final syllables ending in vowels or in *-m*. I hesitate to believe that poetry involved an artificial deviation from prose utterance; yet, on the other hand, I cannot regard the evidence sometimes cited in favor of 'slurring' in prose as in the least decisive.

<sup>2</sup>In my Latin Grammar I nevertheless retained the term (defining it as quantitative prominence), but purely on practical grounds.

1. Professor Hale first objects that my definition of ictus as 'the quantitative prominence inherent in a long syllable' will not hold, because it will not apply to the second long syllable of the spondee when the spondee is substituted for the dactyl in dactylic verse. But the second long of the spondee in such cases is *not* quantitatively prominent. As already pointed out above, the spondee is not a fundamental foot; when it is used as a substitute for the dactyl, it naturally takes on in consciousness the dactylic character, i. e. the quantitative prominence is felt as resting on the first syllable. The second long of the spondee is just as naturally felt to be not-prominent, because it is felt in consciousness as corresponding to the two shorts of the dactyl, which are not quantitatively prominent. In defining ictus as the quantitative prominence inherent in the long syllable of fundamental feet, I by no means say or imply that every long syllable is quantitatively prominent. The situation is precisely the same as in English verse. There we define ictus as the accentual prominence inherent in a stressed syllable. Yet not every stressed syllable is accentually prominent in English verse. In English iambic measures the foot often consists of two stressed syllables; yet the first of these is not felt as accentually prominent, simply because the verse has enough pure iambi to gain a distinct iambic character (˘ ˘) and an occasional spondee (˘ ˘) naturally is felt as prominent only in the second accented syllable.

2. Secondly Professor Hale objects that my view will not hold because in Latin iambic verse the tribrah and dactyl, when substituted for the iambus, take the ictus upon the first of the two short syllables into which the long of the iambus is resolved: ˘ ˘ ˘, — ˘ ˘. This is a clear begging of the question. If it were true that in such cases there was a definite stress on the syllables indicated, there could hardly be further discussion. But that is the very point in controversy. Not a shred of evidence exists to support the theory that the tribrah and dactyl were stressed upon their second syllable in iambic verse. This is frankly acknowledged by Christ in his *Metrik der Griechen und Römer*<sup>2</sup>, p. 55. Christ, to be sure, as well as other authors of manuals of prosody, does accept the hypothesis that the tribrah and dactyl were so stressed in iambic verse. But this view is simply a corollary of the false hypothesis that ictus was stress. Once we assume that the iambus was stressed upon its second syllable, it is not only natural but practically necessary to find a

location for the stress in resolved feet like the tribrach, dactyl, and procelesmatic. But that the ancients put stress either on the long of the iambus or on the syllables into which it was resolved remains to be proved. In fact, to my mind one of the strongest arguments against the stress theory of ictus is that the ancient meticians never allude to the location of the ictus in resolved feet. If ictus was stress and the second syllable of the iambus was stressed in verse, then the location of this stress in resolved feet would be one of the first questions to suggest itself to the meticians. Its consideration would have been inevitable. Yet they never once allude to it, though they enumerate frequently the various possible resolutions of the iambus.

3. Thirdly Professor Hale adduces certain passages from Quintilian which he regards as making for the stress theory of ictus. I cite these in full, italicizing the words which Professor Hale deems important:

IX 4, 51 Maior tamen illic (sc. in rhythmis) licentia est, ubi tempora etiam animo metiuntur et *pedum et digitorum ictu* intervalla signant quibusdam notis atque aestimant quot breves illud spatium habeat;

IX 4, 55 *oratio non descendet ad crepitum digitorum*;

IX 4, 75 sex enim pedes (sc. of the iambic trimeter) *tres percussionses* habent;

IX 4, 136 [iambi] *frequentiorem quasi pulsum* habent.

Professor Hale urges that "verse-pulse is characteristic of all verse-systems of which we have any actual knowledge, and can hardly have been absent from the system of men who by implication speak of verse as lending itself to taps of the fingers and beats of the foot." If by 'verse-pulse' Professor Hale means stress, I must urge again that he is begging the question; if he does not mean stress, I fail to see the point of his remark. Nor does the fact that in the passages above cited Quintilian speaks of verse as lending itself to the taps of fingers and beats of the foot, seem to me to point any more distinctly to a stress rhythm than to a quantitative one. In the case of either it would be perfectly natural to keep the time by taps of the finger or beats of the foot. Professor Hale continues: "not only does the word 'ictus,' like our word 'beat,' naturally imply stress, but it is used as synonymous with percussio in Quint. IX 4, 51 [above cited]; while percussio is used instead of ictus in IX 4, 75 [above cited]."

In answer to Professor Hale's assertion that "ictus naturally

implies stress," I must again urge that that is only a *petitio principii*. Ictus naturally implies stress only to those who start with the assumption that it *is* stress. As pointed out above, the word *ictus* is not a *terminus technicus* of the Latin metricians; so far as I have been able to discover, it is used only twice by the systematic writers on metric, and in these two instances the word *cannot* denote stress. The passages are:

Terentianus Maurus de Metris, 1342 (Keil, VI 366):

una longa non valebit edere ex sese pedem,  
ictibus quia fit duobus, non gemello tempore.

Diomedes, de Pedibus, III (Keil, I 475, 3): ergo una longa pedem non valebit efficere, quia ictibus duobus arsis et thesis, non gemello tempore perquirenda est.

Here, if ictus be taken in the sense of stress, we get the extraordinary doctrine that it takes two stresses, an arsis and a thesis to make a foot. Evidently the word has no such meaning. It means simply beats,—let us say a strong one and a light one. Beat, stroke is the proper meaning (*propria significatio*) of ictus; its figurative meanings can be determined only on the basis of actual usage (as in the two passages just cited), not by *a priori* methods. This applies equally to the word *percussio* as used by Quintilian, IX 4, 75 (above cited). Evidently the word is here figuratively used. Mr. Hale, however, strangely denies this. As I have above quoted his words he says: "ictus is used as synonymous with percussio in Quint. IX 4, 51; while percussio is used instead of ictus in IX 4, 75." In IX 4, 51, however, ictus is used *in proprio sensu* (digitorum et pedum ictu), so that if percussio in IX 4, 75 is used instead of ictus (as in IX 4, 51) it must mean 'taps.' Possibly it does. But 'taps' are not vocal stress.<sup>1</sup> In Quintilian, IX 4, 136 (above cited), Professor Hale declares that Quintilian uses the word *pulsum* in place of *ictus*. If such is the case, I would only observe that, until it is shown that the word *ictus* was used to denote stress, the circumstance that *pulsus* is employed as a synonym of *ictus* is of no significance. Certainly *pulsus* itself does not have that meaning. To me it seems far more

<sup>1</sup> Westphal, Gr. Rhythmik, p. 104, has collected numerous instances of the use of *percussio* by the metricians. He recognizes the word as occurring in the sense of 'interval' and of 'beat,' but, though himself a pronounced adherent of the stress theory of ictus, he is not bold enough to seek in this word any confirmation of that view.

likely that Quintilian is using the word in the same sense in which Marius Victorinus employs it in the following passage (Keil, VI 44, 4): *pes vocatur . . . quia in percussione metrica pedis pulsus ponitur tolliturque.*

4. Fourthly Professor Hale adduces a passage of Charisius in support of the view that ictus was stress. The context in which Charisius used the word is as follows (Keil, I 552, 9): He is discussing gender and observes that some nouns which are masculine in Latin correspond to Greek nouns that are feminine. This observation is followed by a list of fifty or more illustrative examples in alphabetical order: *ingressus εἰσβασις*; *ictus πληγή*; *iuncus, ὀξύσχοινος*, etc. This citation is seriously advanced by Professor Hale in support of the view that *ictus is stress*. I fail to see that it has any bearing upon the question at issue.

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## II.—EXPLANATION OF AN ASSYRIAN CRUX INTERPRETUM.

The word *x̄lu*, which occurs in six passages in the Assyrian historical texts, has never received a satisfactory explanation. It is true that the context, in each case, shows some connection with locality, but nothing of a more definite character can be gathered from this quarter, and the word is not to be found in any of the native vocabularies. Considerable room being thus left for conjecture, it is not surprising that *x̄lu* has been interpreted in various ways.

Joachim Ménant, in his *Annales des Rois d'Assyrie* (1874, p. 37), renders 'bridge,' but this is an obvious conjecture based on the parallel passage *Tig. Pil. IV* 69.

Another French scholar, Joseph Halévy, the leader of the anti-Sumerian school of Assyriologists, cites this word in support of his peculiar theory. In his *Notes de lexicographie assyrienne* (ZK., vol. I, 1884, p. 262) he endeavors to show that Sumerian *xul* 'evil' is really Semitic and connected with Heb. חַוֵּל 'to writhe (with anguish).' In this connection he explains *x̄lu* as an adjective, and thinks that in the passage *Tig. Pil. II* 7-10, where Ménant renders 'bridge,' it actually means 'the bad' (sc. roads).

Professor Friedrich Delitzsch, in his *Wo lag das Paradies?* (1881, p. 259), takes the word as the proper name of an extensive district lying near the Tûr-Abdin Mountains, which he is inclined to identify with the Biblical גִּיא mentioned Gen. 10, 23; 1 Chron. 1, 17, and connects it etymologically with Heb. חַד 'sand.' Professor Delitzsch has long since abandoned this explanation, but has suggested no other, and, in his *Manual Dictionary* (1894), *x̄lu* appears (p. 271<sup>b</sup>) without an accompanying translation.

Lotz, in his *Tiglath-Pileser* (1880), renders doubtfully 'sandy tracts' (Sandstrecken), though he states in a note (p. 121) that he considers the comparison to Heb. חַד, Syr. حَلَّ 'sand' rather uncertain.

However, in default of a better explanation, Assyriologists seem to have agreed, with more or less hesitation, to render *x̄lu*

'sandy place,' 'desert,' 'wilderness.' For example, Hugo Winckler of Berlin, the accomplished editor and translator of the Tel-el-Amarna Tablets, in his translation of the annals of Tiglath-Pileser and of Ašur-nâçir-pal (Schrader's KB., vol. I), renders Tig. Pil. II 9, 'die Wüstniss'; Ašurn. II 96, III 102, and I R. 28, 32b, 'die Wüste.' In Ašurn. III 34, he translates 'böse Stellen,' and explains in a footnote that *xâlu* seems to mean here sandy flats in the river interfering with navigation. It is evident that none of the explanations given rest on very firm ground, and the true meaning of *xâlu* still remains in doubt.

In a letter addressed to King Esarhaddon (Harper's Letters, No. 406), the priestly writer Nabû-axe-erba sees fit to explain words ideographically written by means of interlinear glosses.<sup>1</sup> His reasons for so doing are not obvious. Two, at least, of the ideograms thus explained (*parçu* 'command,' obv. 10, and *mâr-mâre* 'grandchildren,' rev. 13) are quite simple, and would certainly present no difficulty to a modern Assyriologist. It is possible that the glosses were added by another hand. But, whatever the explanation, for our present purpose the idea was a lucky one; for one of these glosses clearly establishes the meaning of the obscure word *xâlu*. The ordinary ideogram (KAS) for *xarrânu* 'road,' which occurs in line 16 of the reverse, is explained by the gloss *xu-u-li* written beneath it. The whole passage reads (rev. 8-19): *ina muxxi Ašur-mukin-paléja, ša šarru beli išpurâni, lillika; tâba ana alâki. Mâr-mârešu šarru beli ina burkešu lintux: ina šiddi KAS (= xu-u-li) LU idqu limuruš*, 'with regard to my lord the king's message about Ašur-mukin-paléja, let him go (by all means); it will be well for him to go. May my lord the king hold his sons' sons on his knees, and see him (going) along the pathway (of life) as a (tender) lamb.' In another passage in Harper's Letters, Nabû-nâdin-šum writes to the king (No. 52, obv. 10-12): *ištu Ninua xu-lu ša <sup>al</sup> Zi ... uktéli* 'I proceeded from Nineveh by the Zi ... road.' It is clear, then, that in these two instances *xâlu* means 'road,' and an examination of the passages in the historical texts, where the word occurs, shows that this meaning holds good in every case. In the annals of Tiglath-Pileser, col. II, 9, the king, after stating how he surmounted the difficulties offered by the mountain-

<sup>1</sup>Cf. the interlinear glosses in the incantation on p. 75 of Professor Haupt's Cuneiform Texts and his remarks, Akkadische Sprache, p. 31, note 22.

passes, continues: *xu-la ana mētiq narkabâtēja u ummânâtēja lâtib* 'I constructed a road for the passage of my chariots and my troops.' In the annals of Ašur-nâçir-pal, col. II, 96, the king, describing the progress of his campaign, says: *ina âlâni ša šiddi xu-li-ja ša libbi šad Kašiari alpe kirre karâni ummâr siparri gurpise siparri attaxar* 'in the cities along my route, in the Kashiari Mountains, I received oxen, sheep, wine, and vessels and *gurpise* (buckets or baskets? stem ܣܰܰ = ܰܰܰ?) of copper.'

Another passage from the annals of the same monarch reads (col. III, 101-102): *ištu māt Malâni attumša ana âlâni ša māt Zamba, ša šiddi xu-li-ja ina išâti ašrup* 'Marching from Malâni to the cities of Zamba, I burnt with fire (all) along my route.' *ša šiddi xâlija* means literally 'that which (was) along my route,' and there is no need to assume with Winckler (KB. I 111) that anything has been omitted by the scribe. In the mutilated text I R. 28, mention is made (l. 32b) of a palace which stood *ša reš xu-li ša al Aššur* 'at the head of the highway to Aššur.'

The difficult passage Ašurn. III 33-34, is rendered by Winckler (KB. I 101): 'Auf Schiffen, die sie gemacht hatten, Schiffe aus Hammelhäuten, welche an bösen Stellen (?) von den Leuten (?) getragen werden (?) überschritt ich den Euphrat.' In a footnote he explains that the sense is probably that the boats, being light, were carried by their crews over the sandy flats of the river. This explanation, which is merely offered by Dr. Winckler as a conjectural interpretation, is quite impossible.

*epušûni* (l. 33) is not 3 pl., but 1 sing., and the final *u* is merely the indication of the modus relativus after *ša*. *elippe ša epušûni* means therefore 'the boats which I had made' and must be taken in connection with l. 29 above: *elippe ša ramenija ina Sûri etâpaš* 'At Sur I constructed my own boats.' In l. 34, not *mašak taxši*, but *mašak gabši* 'inflated skins' must be read, as Prof. Haupt has very clearly shown in his paper, 'Babylonian Words in Ezekiel,' read before the American Oriental Society at its meeting held in Baltimore, April, 1897. Cf. the Notes on Ezekiel 16, 10 in the 'Polychrome Bible,' p. 125, l. 11. *Mašak* seems to be a silent determinative as in *nâdu* (Heb. נָדָע) 'skin bottle.' Both *nâdu* and *gabšu* mean originally 'swollen' or 'inflated.' The preposition *ištu* (l. 34) cannot possibly indicate agency like the Latin *a, ab*. Such a usage is quite unknown to Assyrian.

Apart from the strangeness of the reading *niš-i* for *niše* 'people,' the *i* clearly forms part of the following word, and we must read

*idūlāni* 3 fem. pl., agreeing with *elippe* 'boats.' Prof. Haupt has for years past explained *dālu* to his classes as meaning 'to go about,' 'go around,' and, more recently, Tallqvist, in his *Maqlū* (p. 131), gives the same rendering. This explanation is completely established by examples to be found in Harper's Letters.<sup>1</sup> The only real difficulty of the passage is presented by the character *man, niš* in line 34, and here it is necessary to study the context a little. Ašur-nâcir-pal, having constructed vessels at Sûr on the Khâbûr (l. 29), proceeded down the river as far as its junction with the Euphrates. Here he turned (l. 31), and, going northward, crossed the Euphrates at Kharidu. Line 34 shows that his vessels were in reality rafts composed of a frame-work of wood supported by inflated skins, the prototype, in fact, of the modern *kelek* in use on the Tigris.<sup>2</sup> It is, of course, easy for these vessels to float down stream, but practically impossible to propel them for any considerable distance against the current. At the present day such *keleks*, after reaching Baghdad, are broken up, the timber is sold, and the skins are conveyed back to their starting-point on the backs of asses or camels. Ašur-nâcir-pal evidently refers to some similar procedure when he says (l. 34) that he crossed the river *ina elippe . . . ša ina x̄lli ištu man (niš)* *idulāni* 'in the boats which came round by the road from'—and here follows the character *man, niš*.<sup>3</sup> We should naturally expect the name of a place in this connection, but there appears to be room for this single character alone, and the reading seems to be correct, since the copies of the text that have been found, although they contain a number of variants, agree in this point. It may be that this character has some ideographic value at present unknown, for certainly none of the known values yields sense here. There is, however, another possibility. The two corner wedges forming the character *man* may be read separately, and, in this case, the first *u* could be ideogram for *šupâlu* (Br. List, No. 8749 ff.), while the second would be phonetic complement. But, although this reading yields a good sense in

<sup>1</sup> I expect to treat *dālu* in a future paper. A discussion of it here would unnecessarily prolong the present paper.

<sup>2</sup> See my thesis, 'The Epistolary Literature of the Assyrians and Babylonians,' Part I, p. 169.

<sup>3</sup> Delitzsch, *Manual Dictionary*, p. 213<sup>a</sup>, sees in *man, niš* the common ideogram for 20 and refers to his *Assyr. Lesestücke*, p. 88, l. 19<sup>a</sup>, where we find *elip ešrl* 'a ship of 20 tons.'

the present passage, the orthography would be most unusual, and I offer the explanation merely as a possible conjecture. In any case the meaning of *xâlu* is not affected. The whole passage may then be read: *ina elippe ša epusuni, elippe ša (mašak) gabši ša ina xâlu ištu šupâlû (?) idulâni nân Puratti lâ etébir* 'In the boats which I had constructed, boats of inflated skins which had come around by the road from below (i. e. down the river), I crossed the Euphrates at Kharidu.'

The examples cited would seem to establish the meaning of *xâlu* 'road' beyond any reasonable doubt; the etymology of the word now remains to be considered. There can, of course, be no connection with Heb. **לָן** 'sand,' which is undoubtedly to be compared to Arabic **جَلَّ** 'to change,' and means properly 'changing, shifting ground.' As **لָן** has a **ג**, it would have to appear in Assyrian not as *xâlu*, but as *\*âlu*. A verb *xâlu* is actually to be found in Assyrian, and it means 'to tremble.' Before the might of Shalmaneser *ixilâ mâtâte . . . išdâšina* 'the world trembles to its foundations' (Šalm. Mon., obv. 9). In the incantations the powers of darkness are exhorted *xâlu zâbâ u itâlukâ* 'tremble, dissolve, and vanish' (Tallqvist, Maqlû, I 140; cf. p. 129). The verb *xamâtu*, which means properly 'to tremble, quiver,' and is actually employed in this sense in the incantations (Maqlû, III 30. 168), came later to be applied to rapid motion in general. In the historical texts *xamâtu* means 'to hasten,' and from it is derived the adjective *xamâtu*, *xanâtu* 'swift.' In the case of *xâlu* 'to tremble' precisely the same development of meaning has taken place, and so *xâlu* 'road' forms an exact parallel to another word for road, *urxu*, which is derived from *arâxu* 'to be swift.' The words *xa'âltu* 'army' (i. e. expeditio), and *xi'âlanu* 'troops' are of course derived from this stem, as also Arabic **خَلْ** 'horses, cavalry,' properly 'the swift.' The connection with Hebrew **לָן** 'to tremble' is self-evident; it is also evident that **לָן** 'sand' has here too no etymological connection, and that the explanation given in most Hebrew dictionaries must be modified accordingly.

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### III.—THE CHRONOLOGY OF CICERO'S CORRESPONDENCE DURING THE YEAR 59 B. C.

In the Jahrb. f. class. Philol., Bd. 145, p. 713 ff., there is a brief article by W. Sternkopf entitled 'Ciceros Correspondenz aus den Jahren 59 und 58.'<sup>1</sup> Although the results reached by Sternkopf are valuable, I feel that there are certain difficult questions of primary importance connected with the Correspondence of this period which are still unsettled, and an introductory word or two upon the relation which this article bears to Sternkopf's, set down without any intention of belittling the value of his results, but for the purpose of indicating the scope of my paper and of showing that I have not attempted *rem actam agere*, may not be out of place. Sternkopf has either without hesitation accepted the traditional order as correct chronologically, or he has touched but lightly upon the considerations which help one in determining the chronological sequence. A discussion of the evidence bearing on this part of the problem, while involving much difficulty, is of great importance. I have therefore given special attention to it in the present paper. In the case of a few letters sure conclusions are unattainable because there is a lack of convincing evidence, but, inasmuch as we are bound to adopt *some* chronological order for the letters, I shall be satisfied in such cases if a more probable hypothesis has been substituted for one less probable, or if the traditional view has been placed on a surer basis, or even if attention has been called to evidence which may lead in the future to correct conclusions. The same statement will apply to another feature of this article, that is to the attempt to fix somewhat definitely the dates of the various letters. In this matter it is hoped that, even if the proof is not convincing in all cases for the exact date suggested, the date has been fixed within narrower limits than has heretofore been the case.

The difficulties which beset an investigation of the chronology of the epistles belonging to the year 59 B. C. are perhaps more

<sup>1</sup> This paper was written before Sternkopf's article had been consulted, and its arguments and conclusions are now published without modification.

serious than for some of the other portions of Cicero's Correspondence. Very few of the letters written during that year are dated, and few exact dates are mentioned in them. All the letters of this period, with one exception, were written to Atticus, so that there is no chance of comparing descriptions of the same event in communications addressed to different people. Finally the references which other ancient writers have made to the events of 59 B. C. seem to give little or no help in the matter. The letters of this year are of course of great importance, because they contain the most satisfactory account which we possess, on the one hand, of the first fruits of the coalition formed by Caesar, Pompey and Crassus, and of Caesar's legislation during his first consulship, and on the other hand of the events which led up to Cicero's banishment.

The table which follows indicates the order in which the letters were written and the probable or certain date of each letter, while the arguments upon which the conclusions are based are given in a subsequent part of the article.

*60 B. C.*

Cicero and Atticus meet in Rome, Cicero coming probably from Tusculum, Atticus from Epirus (p. 391), Dec. 29.

*59 B. C.*

Atticus remained in Rome from Dec. 29, 60 B. C., to June, 59 B. C.

It is uncertain where Cicero spent the first 3 months of 59 B. C. Probably he was at Rome part of the time and part of the time at his seaside villas in Latium (p. 392 f.).

Attico II 4 Antio (p. 396 ff.),	Apr. 13.
Attico II 5 Antio (p. 396 ff.),	Apr. 14.
Attico II 6 Antio (p. 396 ff.),	Apr. 15.
Attico II 7 Antio (p. 396 ff.),	Apr. 16.
Attico II 8 Antio (p. 396 ff.), Apr. 17 or evening of Apr. 16.	
Attico II 9 Antio (p. 396 ff.),	Apr. 18.

Cicero leaves Antium and reaches Tres Tabernae at the 10th hour, writing thence

Attico II 12 Tribus Tabernis (p. 398), Apr. 19.  
(although the letter was sent Apr. 20 at some point on the road between Tres Tabernae and Appi Forum).

Cicero goes from Tres Tabernae to Appi Forum, reaching the latter place at the 4th hour and writing

Attico II 10 Appi Foro (p. 399), Apr. 20.

Cicero arrives at Formiae (p. 399),	Apr. 21, evening.
Attico II 11 ex Formiano (p. 399 f.),	Apr. 25 or 26.
Attico II 13 ex Formiano (p. 400),	Apr. 26 or 27.
Attico II 14 ex Formiano (p. 400),	Apr. 27 or 28.
Attico II 15 ex Formiano (p. 400),	Apr. 28 or 29.
Attico II 16 ex Formiano (p. 400 f.),	May 1-5 (probably May 1 or 2).
Attico II 17 ex Formiano (p. 401),	May 2-5 (probably May 3 or 4).

On May 5 Cicero leaves Formiae and goes to his villa at Arpinum, reaching the latter place May 10 (p. 401).

The interval between May 10 and June 1 he spent at Arpinum or at Arpinum and Tusculum. He returns to Rome June 1 (p. 401).

Attico II 18 Roma (p. 401 f.),	June 15-Jul. 6.
Attico II 19 Roma (p. 402),	Jul. 14-25 (certainly after Jul. 6; probably after Jul. 13).
Attico II 20 Roma (p. 402 f.),	Jul. 14-25 (perhaps Jul. 24).
Attico II 21 Roma (p. 403 ff.),	soon after Jul. 25.
Attico II 22 Roma (p. 403 f.),	Jul. 25-Oct. 18 (probably 1st week in Aug.)
Attico II 23 Roma (p. 403 f.),	Jul. 25-Oct. 18 (probably 2d week in Aug.).
Attico II 24 Roma (p. 404 f.),	Jul. 25-Oct. 18 (probably middle of Aug.).
Attico II 25 Roma (p. 405),	Jul. 25-Oct. 18 (probably first half of Oct.).
Q. fr. I 2 Roma (p. 405),	Oct. 25-Dec. 10.

Atticus returned to Rome at the close of the year 60 B. C., apparently on the last day of Dec., for Cicero, writing probably at his Tuscan villa in anticipation of his arrival, says (Att. II 2. 3): *velim . . . , quoniam huc non venis, cenes apud nos* (i. e. at my town house) *utique pridie Kal.* This letter was written toward the close of the month, as we can see from a remark in the same section: *sed heus tu ecquid vides Kal. venire, Antonium non venire?*, and reference is made to the kalends of January, as is evident from a letter written a few days later, for, in speaking apparently of the same dinner engagement, he says (Att. II 3. 3): *sed haec ambulationibus Compitaliciis reservamus. Tu pridie Compitalia memento.* The Compitalia occurred very soon after

the Saturnalia; under the emperors the festival began Jan. 3 (Marquardt, *Röm. Staatsverwaltung*, III, p. 206), but in the period under consideration the exact date was not fixed, and apparently in 59 B. C. they fell on Jan. 1, as they certainly did in 61 B. C. (cf. Cic. in Pis. 8). With the exception of a few days spent at Arpinum in May (cf. p. 401), Atticus was probably in Rome until June, 59 B. C., perhaps for the purpose of obtaining from the senate or the consuls papers which would enable him to collect money due him at Sicyon (cf. Att. I 19. 9; II 13. 2). Then he left the city to go to Epirus, for in the early part of July, Cicero, who was at that time in Rome, acknowledged the receipt of several letters from his friend (Att. II 18. 1), while from a previous letter we know that Atticus was in Rome until May 8 at least, since he had promised to go thence to Arpinum about May 10; cf. Att. II 17. 1 *haec in Arpinati a. d. vi circiter Id. Mai. non deflebimus*, although Cicero was very much afraid that business affairs would prevent Atticus from keeping his engagement, and would detain him in Rome until his own return to the city; cf. Att. II 17. 3 *tu tamen videris mihi Romae fore ad nostrum adventum*.

These references determine in the main the movements of Cicero also during the early part of 59 B. C. In Dec., 60 B. C. he writes (Att. II 3. 3): *venio nunc ad mensem Ianuarium et ad ἵπποστασιν nostram ac πολιτείαν . . .* *Est res sane magni consili.* *Nam aut fortiter resistendum est legi agrariae . . . aut quiescendum, quod est non dissimile atque ire in Solonium aut Antium, aut etiam adiuvandum.* Cicero's references in 59 B. C. and in subsequent years to Caesar's agrarian laws make it almost certain that he neither openly opposed nor supported those laws. Furthermore, no ancient writer mentions Cicero in connection with the matter except Plutarch, who tells us (Cat. Min. 32) that Cicero advised Cato to promise under oath to observe Caesar's law. Plutarch's statement will be considered later. The agrarian measures of this year excited such intense political feeling (Dio Cass. XXXVIII), and Cicero was a man of so much political prominence that, if he desired to hold himself aloof from the discussion, it would have been necessary for him, as he himself felt (cf. citation above from Att. II 3. 3), to absent himself from Rome. General probability therefore points very distinctly to the hypothesis that he was not in Rome during the agitation connected with the passage of the laws mentioned.

Four facts seem to militate against this theory: first, Cicero's plan to be in Rome during the Compitalia; second, Plutarch's statement as quoted above; third, the delivery of an oration by Cicero in defence of C. Antonius during the early part of the year; and fourth, the absence of any letters to Atticus, who was at Rome, during the months of January, February and March. As for his presence at Rome during the Compitalia, since no meetings of the senate could be held during that festival, Cicero could remain in the city with impunity. He could stay safely at Rome also during the *dies comitiales* in January, i. e. Jan. 3-4 and 16-29, when likewise no meetings of the senate could be held (cf. Willems, *Le Sénat de la République romaine*, II 152). Furthermore, the conciliatory attitude which Caesar adopted during the early part of his consulship makes it highly probable that he did not even publish his agrarian bill during the month of January (cf. Drumann, *Geschichte Roms*, III 195-6; Lange, *Röm. Alterthümer*, III 279). Finally, after the passage of the *lex Gabinia* of 61 B. C. the senate devoted the entire month of February to the reception of foreign embassies (Willems, II, p. 156). It is quite possible, therefore, that Caesar's agrarian bill was not brought forward until March. At all events, Cicero could stay at Rome during the greater part of the first two months of the year 59 B. C. without fear of being called upon to take part in an agrarian discussion. During this period Antonius' trial was held (cf. Att. II 7. 2; *de dom.* 41), and Cicero's presence in Rome would account for the absence of any letters to Atticus during the months of January and February at least. As for Plutarch's statement, Cicero's advice to Cato may well have been given by letter. Cicero was, however, certainly absent from Rome, at one or another of his country-seats, from the middle of April (cf. p. 398 ff.), and probably from the beginning of March, until June 1; cf. Att. II 8. 2 *inde cogito in Tusculanum, deinde Arpinum, Romam ad Kal. Iun.*

In considering the earliest letters of the year 59 it will be convenient first to establish the chronological order, then to determine the place of writing, and finally to fix the dates of the various letters in so far as it is possible.

Of the epistles to be discussed, letters 4, 5, 6, 7, 8, 9, 12 and 10 may be considered together and, as we shall try to show, were probably written in the order indicated. In Att. II 4. 2 Cicero writes: *interea quidem cum Musis nos delectabimus animo aequo,*

immo vero etiam gaudenti ac libenti; neque mihi umquam veniet in mentem Crasso invidere neque paenitere, quod a me ipse non desciverim, whereas in II 5. 2, as if with this statement in mind and as if to modify it, he says: *de istis rebus exspecto tuas litteras: . . . cuinam auguratus deferatur, quo quidem uno ego ab istis capi possum.* Vides caritatem meam! Sed quid ego haec, quae cupio deponere et toto animo atque omni cura φιλοσοφεῖν? Sic, inquam, in animo est. Vellem ab initio. Nunc vero, quoniam quae putavi esse praeclera expertus sum quam essent inania, cum omnibus Musis rationem habere cogito. In Att. II 5. 3 Cicero asks: *quid de P. Clodio fiat*, and it may be argued that this question must precede the remark in II 4. 2 *Clodius ergo, ut ait, ad Tigranem?* It may well be, however, that in II 5. 3 Cicero is asking for more definite information upon this very appointment of Clodius. Therefore, although the matter cannot be settled beyond the reach of doubt, it seems proper to accept the traditional order, i. e. to place 4 before 5.

As for Att. II 6, it was written later than II 4. When II 4 was written Cicero had just received from Atticus Serapio's work on geography (*fecisti mihi pergratum, quod Serapionis librum ad me misisti*, Att. II 4. 1), whereas at the time of writing II 6 he had read the book and was considering the criticisms upon Eratosthenes which it contained. Furthermore, the first sentence in Att. II 6 (*quod tibi superioribus litteris promiseram, fore ut opus extaret huius peregrinationis*) refers to Cicero's remark in Att. II 4. 3 *de geographia dabo operam ut tibi satis faciam.* Cf. also II 6 (end) and II 4 (end). Att. II 5 is concerned exclusively with political questions, while II 6 is devoted to private matters, so that there is no point of contact between the two letters, and consequently little opportunity to find material for determining their sequence. However, the calm tone of II 6 and the absence of any reference to political matters make it almost certain that II 6 follows II 5.

Att. II 7 follows II 6; cf. Att. II 6. 1 (*quod tibi superioribus litteris promiseram, fore ut opus extaret huius peregrinationis, nihil iam magno opere confirmo*) with II 7. 1 (*de geographia etiam atque etiam deliberabimus. [and then, after mentioning the possibility of his working upon certain orations] . . . Denique aliquid exstabit, ne tibi plane cessasse videamur*). It may be noted in this connection also that the references in Att. II 4. 7; 6. 2 and 7. 5 to the repair of a certain wall running between the

premises of Marcus and Quintus Cicero on the Palatine indicate that these three letters were written at short intervals.

When he wrote Att. II 7 Cicero had heard from the younger Curio that the position of the triumvirs was not so secure as it had been; cf. sec. 3 *una spes est salutis istorum inter istos dissensio, cuius ego quaedam initia sensi ex Curione.* When II 8 was written he had received a letter from Atticus confirming the impression which Curio had given him; cf. sec. 1 *et scito Curionem adulescentem venisse ad me salutatum.* Valde eius sermo de Publio cum tuis litteris congruebat. *Ipse vero mirandum in modum reges odisse superbos.* Peraeque narrabat incensam esse iuventutem neque ferre haec posse. Att. II 8 is therefore later than II 7.

Att. II 9 is later than II 8. In II 8. 2 Cicero writes: *Kal. Mai. de Formiano proficiscemur, ut Anti simus a. d. v. Non. Mai. . . . Inde cogito in Tusculanum,* while at the time of writing II 9 he has been induced by some considerations unknown to us to postpone the date of his departure from Formiae, and he is also able to fix exactly the date of his intended departure from Antium: *Antium me ex Formiano recipere cogito a. d. v. Non. Mai. Antio volo Non. Mai. proficisci in Tusculanum* (Att. II 9. 4).

Att. II 12 was written at Tres Tabernae, April 19 (cf. p. 398), but was not apparently delivered to a messenger until the next day; cf. Att. II 12. 2 *emerseram commodum ex Antiatii in Appiam ad Tris Tabernas, ipsis Cerealibus, cum in me incurrit Roma veniens Curio meus.* *Ibidem ilico puer abs te cum epistulis,* with sec. 4 *litteras scripsi hora decima Cerealibus, statim ut tuas legeram, sed eas eram daturus, ut putaram, postridie ei, qui mihi primus obviam venisset, and II 10 dederam aliam* (i. e. Att. II 12) *paulo ante a Tribus Tabernis.* Att. II 10 was one day later than II 12 (see p. 399).

The letters of this group (Att. II 4, 5, 6, 7, 8, 9, 12 and 10) were, therefore, probably written in the order indicated.

Of the letters which we have been discussing, Att. II 4, 5, 6, 7, 8 and 9 were written at Antium. This conclusion seems certain from the facts which are now to be stated. In the third letter of this group, viz. II 6, Cicero speaks of being at Antium (cf. sec. 1), while in II 12, which, as we have shown, follows II 9 and was written at Tres Tabernae, he says: *emerseram commodum ex Antiatii in Appiam ad Tris Tabernas, etc., Att. II 12. 2.* Furthermore, in II 8. 2, in announcing his plans, he writes: *in Formianum*

volumus venire Parilibus; inde . . . Kal. Mai. de Formiano proficiscemur, ut Anti simus a. d. v. Non. Mai., and in II 9. 4 Antium me ex Formiano recipere cogito a. d. v. Non. Mai. Antio volo Non. Mai. profici in Tusculanum. Sed cum e Formiano *rediero*, etc. A comparison of these two passages makes it clear that the point from which Cicero will set out, and to which he will *return*, and consequently the point from which both letters were sent, was Antium. It is certain, then, that at least three letters of the series 4, 5, 6, 7, 8, 9 were written at Antium. But since Cicero is very anxious not to miss any of his friend's letters, and hopes also that Atticus may find an opportunity to pay him a visit, he keeps him carefully informed with reference to any change of residence which he has in mind (cf. Att. II 8. 2; II 10). Now, in 4, 5, 6, 7, 8, 9 there is no indication that such a change has been made. We may conclude, therefore, that all these letters were written at the same place, and since Cicero was at Antium when three of them (6, 8 and 9) were written, we are safe in saying that all of them were written at that place. All the editors of Cicero's Correspondence assign Att. II 4 to Tusculum, but the evidence, in so far as I can see, is against that hypothesis.

Att. II 12 was written at Tres Tabernae, as shown above; cf. II 12. 2 emerseram commodum ex Antiat in Appiam ad Tris Tabernas, ipsis Cerealibus . . . Ibidem ilico puer abs te cum epistulis, with sec. 4 litteras scripsi hora decima Cerealibus, statim ut tuas legeram, etc. As for Att. II 10, it is dated at Appi Foro.

Passing now to a consideration of the dates of the particular letters under discussion, let us at the outset determine the month to which the earliest letter (Att. II 4) belongs. In sec. 6 of that letter Cicero says: nos circiter Kal. aut in Formiano erimus aut in Pompeiano. We can safely assume that the writer refers to the kalends of the following month. Otherwise the month would be indicated. A comparison of this passage with Att. II 8. 2 shows that Cicero has in mind the kalends of May, for in the latter passage he says: in Formianum volumus venire Parilibus; inde . . . Kal. Mai. de Formiano proficiscemur, and in II 9. 4 he remarks: Antium me ex Formiano recipere cogito a. d. v. Non. Mai. Att. II 10 leads one to make the same inference: qua re usque ad Non. Mai. in Formiano exspectabo, and II 11. 2; 13. 2; 14. 2 and 15. 3 point to the same conclusion. Probably Atticus had intimated that he might visit him about the first of the next month, and had inquired where Cicero would be at that time.

To this inquiry Cicero replied in II 4. 6. This view of the matter is strongly confirmed by II 15. 3 (written toward the end of April, as will be shown later): *quoniam tu certi nihil scribis, in Formiano tibi praestoler usque ad a. d. iii Nonas Maias.* Att. II 4 belongs undoubtedly then to the month of April, and incidentally it has been shown that other letters of this group were written in the same month.

In discussing below (see p. 399) the exact dates of various letters, the fact is established that II 10, the last letter of the group under consideration, was written Apr. 20. Now, since II 4, the earliest letter of this group, was also written in April, it is clear that the entire series of letters falls between Apr. 1 and 21. Some considerations which follow make it probable, though not certain, that these letters were written day by day in the interval between April 12 and 21.

Antium, Tres Tabernae, and Appi Forum, at which places these eight letters were written, could be easily reached from Rome by a letter-carrier in a day. In Att. II 13. 1 Cicero says, in fact: *at scito eum fasciculum, quo illam conieceram domum* (i. e. to Rome from Tres Tabernae) *eo ipso die latum esse, quo ego dederam.* Now, Cicero's interest in politics was so lively that Atticus apparently sent him a letter from Rome every day during this period, for Cicero writes to him (Att. II 8. 1): *epistulam cum a te avide exspectarem ad vesperum, ut soleo,<sup>1</sup> ecce tibi nuntius pueros venisse Roma, and after he had left Antium he expressed his regret at losing these daily missives: dies enim nullus erat, Anti cum essem, quo die non melius scirem Romae quid ageretur quam ii qui erant Romae.* Etenim litterae tuae non solum quid Romae, sed etiam quid in re publica, neque solum quid fieret, verum etiam quid futurum esset indicabant, Att. II 11. 1, and in Att. II 12. 2 he refers to the knowledge of the *ruminaciones cotidiana* which the letters of Atticus brought him.

In view of Cicero's lively interest in the course of politics at Rome, and in view of the life of absolute leisure which he was leading (cf. Att. II 6), it is almost certain that these daily letters of Atticus would call forth daily answers from him. What has been said applies particularly, as noted above, to the period of Cicero's stay at Antium, i. e. to the time within which the letters, Att. II 4-9, were written. If we can therefore determine the date

<sup>1</sup> *ut soleo* seems to indicate that Cicero looked for a letter every evening.

on which any one of the letters just mentioned was written, we shall be able to fix the date of all of them with considerable probability. Fortunately, this can be done. In Att. II 8. 1, in a passage already quoted in part, Cicero writes: *epistulam cum a te avide exspectarem ad vesperum, ut soleo, ecce tibi nuntius pueros venisse Roma. Voco; quaero, ecquid litterarum? . . . Perterriti voce et vultu confessi sunt se accepisse, sed excidisse in via. . . . Nunc, si quid in ea epistula, quam ante diem xvi. Kal. Mai. dedisti, fuit historia dignum, scribe quam primum, ne ignorremus.* The lost letter of Atticus was dated Apr. 15. The messengers would start early in the morning of the next day and traverse the distance between Rome and Antium, about 30 miles, in one day, reaching the latter place, as Cicero says, *ad vesperum* of Apr. 16. Att. II 8, advising Atticus of the loss of his letter, would be written at once, either in the evening of April 16 or on the following morning, and dispatched early in the morning of April 17. Working backwards and then forwards, we reach the conclusion that Att. II 7 was written April 16, II 6 April 15, II 5 April 14, II 4 April 13, and II 9 April 18. This calculation is confirmed by statements, already cited in another connection, in Att. II 12. 2 *emerseram commodum ex Antiatu in Appiam ad Tris Tabernas, ipsis Cerealibus, cum in me incurrit Roma veniens Curio meus, and in Att. II 12. 4 litteras scripsi hora decima Cerealibus, statim ut tuas legeram, sed eas eram datus, ut putaram, postridie ei, qui mihi primus obviam venisset.* The *ipsa Cerialia* were April 19. Cicero left Antium, then, early in the morning of April 19, reached Tres Tabernae at the 10th hour, and at once wrote Att. II 12, continuing thus the series of daily letters which, as we have surmised, began with Att. II 4.

The view expressed in the preceding pages receives some support from evidence quite independent of that already mentioned. If it is true that Cicero and Atticus each wrote daily letters during the period under consideration, we shall expect to find a closer relation existing between alternate than between successive letters. We have tried to prove, for instance, that II 5 was sent from Antium on the morning of April 14. In that case it would reach Atticus at Rome in the evening of the same day, and the letter which Atticus sent on the morning of April 15 would be the reply to II 5. That reply would reach Cicero the same day (April 15), and II 7 in a certain sense would be written in answer to the letter of Atticus. We should then expect to find

a somewhat close relation existing between II 5, Atticus's letter of April 15, and II 7. As the letters of Atticus are not extant, the connecting link between 5 and 7 is lost, but, notwithstanding that fact, it is noticeable that 5 and 7 have much more in common than 4 and 5 or 5 and 6, or 6 and 7. Compare, for instance, the reference to Clodius in 5. 3 and 7. 2, to Arrius in 5. 2 and 7. 3, to the augurate in 5. 2 and 7. 3. In each of these cases the passage in II 7 sounds as if it were a comment upon the reply which Atticus had made to Cicero's remark upon the same subject in II 5. A similar relation, though less marked, exists between 4 and 6, 6 and 8, 7 and 9; cf., for instance, 4. 3 and 6. 1, 4. 7 and 6 (end). This state of things cannot well be explained on any other theory than the hypothesis that the letters sent from Antium were written on successive days.

But to return to Cicero's movements after leaving Tres Tabernae,—he was on his way to Formiae, and his next letter to Atticus (Att. II 10) was written at Appi Forum; cf. II 10 *Ab Appi Foro hora quarta*. *Dederam aliam* (i. e. II 12) *paulo ante a Tribus Tabernis*. This remark must refer to the sending of the letter written in the afternoon of the preceding day, i. e. written *hora decima* (cf. II 12. 4) of April 19. Since this letter (viz. II 12) was to be sent on the following day (cf. *eas eram daturus, ut putaram, postridie, etc.*, Att. II 12. 4), it is evident that II 10 was written April 20. The distance from Appi Forum to Formiae was about 50 miles. Horace and his friends, travelling somewhat slowly, traversed it in two days. Cicero occupied a half day in going from Tres Tabernae to Appi Forum, a distance of 18 miles, so that in all probability he reached Formiae the evening of April 21. This conclusion harmonizes with a statement made in a previous letter (Att. II 8. 2): *in Formianum volumus venire Parilibus*.

Another group of letters comprises Att. II 11, 13, 14, 15 and 16. The first two letters in this series also are the difficult ones to arrange with certainty, and it is possible that 13 is earlier than 11, but the weight of evidence is in favor of the traditional order. In II 11. 1 Cicero laments the fact that he gets no Roman news at Formiae except that which comes from passing travellers, whereas when II 13 was written he had received a letter from Atticus (cf. sec. 2). The opening sentences of II 11 also contrast the state of things in Formiae with that in Antium, and indicate pretty plainly that this is the first letter from Formiae. The

opening sentences, however, indicate that Cicero had been at Formiae several days before writing, since he says: *narro tibi: plane relegatus mihi videor, postea quam in Formiano sum. Dies enim nullus erat Anti cum essem, quo die non melius scirem Romae quid ageretur quam ii qui erant Romae. . . . Nunc, nisi si quid ex praetereunte viatore exceptum est, scire nihil possumus.* These remarks make it probable that Cicero had been at Formiae at least four or five days, and indicate, therefore, that the letter cannot well have been written before April 25 or 26. We cannot give it a later date, as we shall soon see, without assuming that Cicero wrote two letters per day during the latter part of this month, which is improbable. This last consideration makes it necessary also to assign II 13 to April 26 or 27.

There is little to help one in determining the relative positions of II 13 and 14. However, in II 13. 2, reiterating a statement contained in II 11. 2, Cicero says: *tu si ad Sicyonios litteras habes, advola in Formianum unde nos pridie Nonas Maias cogitamus.* Now in II 14 reference is made to the same fact, but in a way to indicate that it had already been communicated to Atticus: *statim mehercule Arpinum irem, ni te in Formiano commodissime exspectari viderem dumtaxat ad prid. Nonas Maias* (sec. 2). There is no reason to question the accepted order then, and II 14 was probably written April 27 or 28.

Att. II 15 is later than II 14, as we see by a comparison of Att. II 14. 2 *statim mehercule Arpinum irem, ni te in Formiano commodissime exspectari viderem dumtaxat ad prid. Nonas Maias*, and II 15. 3 *quoniam tu certi nihil scribis in Formiano tibi praestoler usque ad a. d. iii Nonas Maias.* In the first passage Cicero expresses some hope of seeing Atticus at Formiae, but a subsequent letter from him, perhaps the letter to which reference is made in the first sentence of II 15, has shown him that the plans of Atticus are very indefinite, so that he fixes an earlier date for his departure from Formiae. This letter was written before Cicero received the important letter *de agro Campano*, for he makes no mention of that matter in it. Att. II 16 was delivered to him in the afternoon of April 29 (cf. Att. II 16. 1). Therefore Att. II 15 should probably be dated April 28. It is this fixed point in the chronology of the letters from Formiae which proves that 11, 13, 14 and 15 cannot well have been written later than the dates given them above. Att. II 16 is in reply to a letter upon the Campanian land bill which reached Cicero at

Formiae April 29, shortly after the *cena*. Cicero expected to leave Formiae about May 5; cf. Att. II 15. 3 in Formiano tibi praestoler usque ad a. d. iii Nonas Maias. It had been his plan for some time to leave Formiae shortly before the nones of May (cf. II. 2; 13. 2; 14. 2), in order that he might be in Arpinum to receive Atticus about May 10 (cf. II 17. 1), and there is no reason to believe that this plan was changed. Att. II 16 and 17, both letters from Formiae, were therefore written between Apr. 29 and May 5, and as Cicero would seem to have considered carefully the report of Atticus concerning Caesar's agrarian law (cf. Att. II 16. 1), probably II 16 was not written before May 1 or 2, and II 17 still later, since Cicero was just about to leave Formiae (cf. Att. II 17 [end] and 3 [beginning]), but of course before May 5.

To sum up the conclusions which have been reached with reference to the letters from Formiae. The letters of this group —viz. Att. II 11; 13; 14; 15; 16 and 17—were written in the order indicated. The first four fall between the evening of April 21, the date of Cicero's arrival at Formiae, and the evening of April 29, when he first heard the details of Caesar's Campanian law. The probable dates for them are: Att. II 11, April 25 or 26; II 13, April 26 or 27; II 14, April 27 or 28; and Att. II 15, April 28 or 29. Att. II 16 and 17 were written between April 29 and May 5, the former probably on May 1 or 2, the latter, May 3 or 4.

In April Cicero had written to Atticus: *inde* (i. e. from Antium) *cogito in Tusculanum, deinde Arpinum, Romam ad Kal. Iun.*, Att. II 8. 2. This letter was written when Cicero intended to return to Antium from Formiae in time for the games at the former place, but afterward he decided not to attend the games, and so went north from Formiae rather than from Antium, going first to Arpinum rather than to Tusculum. There is no reason to believe that his general plan was changed, however, and probably from May 10 to the end of the month he remained at Arpinum and Tusculum. June first found him in Rome again (cf. II 8. 2). Atticus left Rome for Epirus probably about this time.

The first letter from Cicero after the departure of Atticus was Att. II 18. It was written at Rome and must therefore be later than June 1. Since the departure of Atticus, Cicero has received several letters from him (*accepi aliquot epistulas tuas*, Att. II 18. 1), so that II 18 should probably be dated later than June 15. No mention is made of the demonstration against Pompey at the

Ludi Apollinares, Jul. 6-13 (cf. Att. II 19. 3), so that those games had not yet been given, and the letter is therefore earlier than Jul. 13. Furthermore, Cicero writes (Att. II 18. 2): *habet etiam Campana lex exsecrationem in contione candidatorum, si mentionem fecerint, quo aliter ager possideatur atque ut ex legibus Iuliis. Non dubitant iurare ceteri; Laterensis existimatur laute fecisse, quod tribunatum pl. petere destitit, ne iuraret.* Mommsen has pointed out (St. R. I, p. 620 and n. 5) that this oath was taken on occasion of the formal *professio*, when official announcement was made of the list of candidates; cf. also Madvig, *Verfassung u. Verwaltung*, I 253. This list was made up a *trinundinum* (i. e. 17 days, cf. Herzog, St. Verf. I 1092, n. 2) before the election took place (cf. Momms., St. R. I, p. 502; Herzog, St. Verf. I, p. 656). It will be shown later in this article that in the year 59 the election was probably held July 23 or 24. As the *professio* had not taken place when Att. II 18 was written (cf. *non dubitant iurare ceteri*, etc., in the passage quoted above), that letter must be dated earlier than July 6. It was therefore written apparently between June 15 and July 6, probably during the latter half of June.

Att. II 19 was written after July 6, and probably after July 13, as shown by the reference to Pompey's reception at the *Ludi Apollinares*, but probably before July 25, since no mention is made in it of an event occurring July 25, which also illustrated Pompey's unpopularity (cf. Att. II 21. 3).

Att. II 20 is later than II 19, for in the latter epistle Cicero says (sec. 5): *in iis epistulis me Laelium, te Furium faciam; cetera erunt ἐν αἰνῆσιν*, whereas in II 20 he remarks (sec. 5): *quod scripseram me te Furium scripturum, nihil necesse est tuum nomen mutare. Me faciam Laelium et te Atticum, etc.*

Att. II 20 is therefore later than July 13, but it must have been written before July 25. Perhaps the following considerations enable us to fix its date more definitely. In Att. II 20. 6 reference is made to the postponement of the *comitia* by Bibulus. In Att. II 21. 5 Cicero writes: *Bibuli qui sit exitus futurus nescio. Ut nunc res se habet, admirabili gloria est: qui cum comitia in mensem Octobrem distulisset, quod solet ea res populi voluntatem offendere, putarat Caesar oratione sua posse impelli contionem, ut iret ad Bibulum: multa cum seditiosissime diceret, vocem exprimere non potuit. Quid quaeris? Sentiunt se nullam ullius partis voluntatem tenere.* It would seem probable that the

*contio* held July 25, in which Pompey inveighed against the proclamations of Bibulus (cf. Att. II 21. 3 *non tenui lacrimas, cum illum* [i. e. Pompeium] *a. d. viii. Kal. Sext.* vidi de edictis Bibuli contionantem), was the assembly in which Caesar spoke, and that the postponement of the elections by Bibulus was the subject of Pompey's invective also. If this be the case, in view of the fact that the *contio* was probably called while the popular indignation was still hot against Bibulus, we shall not be far wrong in assuming that the proclamation of Bibulus postponing the elections was published only a day or two before the *contio*, i. e. a day or two before July 25, perhaps July 23 or 24. Now, Att. II 20 is subsequent to the publication of the proclamation (cf. Att. II 20. 6), but apparently before the *contio*; therefore it was probably written July 23 or 24.

It may be noted incidentally that these facts enable us to determine somewhat definitely the date at which the *comitia* for the election of consuls would be held at this period, if not postponed. In 59 B. C. they were certainly held between July 13 and 25, and quite possibly July 23 or 24. For the date in certain other years, cf. Herzog, Röm. Staatsverfassung, I 654; Mommsen, Staatsrecht, I 584, n. 5.

As we have remarked above, II 20 antedates the *contio* to which reference is made in II 21. 5, so that II 21 is later than II 20. Furthermore, since in II 20. 6 the exact date is mentioned to which the elections were postponed, it is evident that Cicero is communicating a fresh item of news, whereas in II 21. 5 the same event is referred to incidentally as a fact already known.

It is impossible to establish with certainty the chronological order of II 21 and II 22, but in a negative way it may be said that there is no ground for changing the traditional order, and from a positive point of view the tone of II 22 indicates that it was subsequent to II 21. In Att. II 22. 7 Cicero writes: Libros Alexandri, neglegentis hominis et non boni poetae, sed tamen non inutilis, tibi remisi. Numerium Numestium libenter accepi in amicitiam et hominem gravem et prudentem et dignum tua commendatione cognovi. This reference to the *libri Alexandri* and to Numerius connects the letter with Att. II 20, since in sec. 1 of that letter Cicero mentions the arrival of Numerius with a letter of recommendation from Atticus and in sec. 6 he acknowledges the receipt of certain books, probably the works of Alexander. These statements do not enable us to fix the date of II

22 with any exactness, but they indicate that no long interval had elapsed between the writing of II 20 and II 22. Now, the former was written July 23 or 24, so that II 22 probably belongs to the early part of August.

The subject-matter, the tone and the phraseology of Att. II 22 and II 23 indicate that these two letters were separated by a very short interval. In both letters mention is made of the threatening attitude of Clodius (22. 1; 23. 3), of the attempt which Pompey was making to restrain him (22. 2; 23. 3), of Pompey's success, which was only apparent (22. 2; 23. 3), of the fact that Cicero was holding aloof from politics, but was very busy in the courts (22. 3; 23. 1 and 3), of Pompey's regret for his course in politics (22. 6; 23. 2), of the solidarity of the conservative elements (22. 3; 23. 2), of the necessity for the presence of Atticus (22. 4; 23. 3). These ideas are expressed in the same words in many cases in the two letters, e. g.: *taedet ipsum Pompeium vehementerque paenitet* (22. 6); *te scire volo, Sampsiceramum . . . vehementer sui status paenitere* (23. 2); *multis denuntiat* (22. 1); *non mediocres terrores iacit atque denuntiat* (23. 3): (when urging Atticus to come to Rome at once) *quid rei magnitudo postulet intellegis* (22. 5); *magnitudo rei longam orationem fortasse desiderat*, etc. (23. 3). Only a few days can have elapsed, then, between the writing of Att. II 22 and II 23, so that the latter was probably written before the middle of August, perhaps between Aug. 7 and 15.

In Att. II 24 Cicero writes: *quas Numestio litteras dedi, sic te iis evocabam, ut nihil acrius neque incitatius fieri posset* (sec. 1). The letter to which reference is here made is apparently Att. II 23 (cf. sec. 3 *quam ob rem, si me amas tantum, quantum profecto amas; si dormis, expergiscere; si stas, ingredere*, etc.). Att. II 24 therefore follows II 23 closely and was probably written about the middle of August. The fact that the conspiracy of Vettius had just become known when II 24 was written *does* enable us, however, to fix a date before which that letter and all the preceding letters must have been written. The revelations of Vettius must have been made before the consular elections (Oct. 18), because in his invective against Vatinius Cicero charges Vatinius (in Vat. 25) with having induced Vettius to lodge information against L. Lentulus, who was at the time a candidate for the consulship (*quod erat eo tempore Gabini competitor*). In fact all of the letters under discussion,—viz. 21, 22,

23, 24 and 25,—must have been written before Oct. 18, the date of the elections, since no mention is made in them of the result of those elections.

We reach the general conclusion, then, that Att. II 21 was written soon after July 25, that II 22, 23 and 24 fall between that date and the middle of August, while II 25 probably belongs to the early part of October, since it was written after the trial of Flaccus, or at all events after Hortensius had delivered his oration in behalf of Flaccus (cf. Att. II 25. 1), but before the deferred elections of Oct. 18.

One consideration may throw some doubt on this conclusion. In II 23. 3 Cicero writes: *nos autem . . . publicis consiliis nullis intersumus totoque nos ad forensem operam laboremque contulimus. Ex quo, quod facile intellegi possit, in multa commemoratione earum rerum, quas gessimus, desiderioque versamur.* The reference in this passage is undoubtedly to Cicero's defence of Flaccus, who had been praetor in 63 B. C. Now, from pro Flacco 96 it seems probable that the *Or. pro Flacco* was delivered after the revelations of Vettius had been made. In II 24 a long account is given of the conspiracy of Vettius as though it were a fresh item of news. This circumstance taken by itself would lead one to give II 23 a later date than II 24, but in view of what has been said on the other side, it seems wiser to leave the traditional order unchanged, and to explain the difficulty by assuming that when Att. II 23 was written Cicero was merely preparing his material for the defence of Flaccus.

Q. fr. I 2, the last letter of 59 B. C.,<sup>1</sup> was written after Oct. 25 (cf. sec. 1) but before Dec. 10, since the tribunes had not yet entered upon their office; cf. sec. 15 *tribuni pl. designati*.

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<sup>1</sup> *Fam. XIII 42 and 41*, which are commonly assigned to 59 B. C., probably belong to the year 58 (cf. Koerner and Schmidt in Mendelssohn, *M. Tulli Ciceronis Epist.*, p. 449).

#### IV.—THE TABULA VALERIA.

In a letter to Terentia (ad Fam. XIV 2. 2) written from Thessalonica during his exile, Cicero says: *A te quidem omnia fieri fortissime et amantissime video, nec miror, sed maereo casum eius modi ut tantis tuis miseriis meae miseriae subleventur. Nam ad me P. Valerius, homo officiosus, scripsit, id quod ego maxima cum fletu legi, quem ad modum a Vestae ad tabulam Valeriam ducta essem.*

The meaning of this phrase has always been a disputed point, and it is the purpose of this paper to discuss the opposing views with some of the arguments advanced on either side. Mommsen, Jordan, Gilbert, Tyrrell and the various editors of Cicero have stated their opinion of its meaning, but so far as I know there is no such discussion of the matter in print.

The general thought of the passage in question is plainly this, that Terentia was forced to undergo some indignities at the hands of the persecutors of her husband. It is probable too, from the context, that this refers to some sort of financial transaction, but whether a declaration of the amount of money Cicero possessed, the amount Terentia herself had, what means Cicero may have taken to evade the rigor of the confiscation, or whether Terentia goes to some banker to borrow, is left wholly uncertain. We turn first to the other occurrence of the phrase for light, and read in the interrogatio in Vatinium 21, where Cicero is attacking Vatinius for his conduct towards the consul Bibulus: *volo uti mihi respondeas, cum M. Bibulum consulem non dicam bene de re publica sentientem, ne tu mihi homo potens irascare, qui ab eo dissensisti, sed hominem certe nusquam progredientem, nihil in re publica molientem, tantum animo ab actionibus tuis dissentientem, cum eum tu consulem in vincula duceres et a tabula Valeria collegae tui mitti iuberent, fecerisne ante rostra pontem continuatis tribunalibus, per quem consul populi Romani moderatissimus et constantissimus sublato auxilio, exclusis amicis, vi perditorum hominum incitata turpissimo miserrimoque spectaculo non in carcerem sed ad supplicium et ad necem duceretur.*

This seems to mean that in the year 59 B. C. Vatinius, then a tribune, had seized Bibulus and attempted to throw him into prison, and to prevent any rescue by Bibulus' friends, he had made a sort of raised way through the Forum out of the various *tribunalia* to be found there. The other tribunes—a *tabula Valeria*—had ordered him to release the consul.

On this passage we have the following note in the *Scholia Bobiensia*: *hi collegae intercesserant P. Vatinio furenti M. Bibulum in invidiam duci* (or according to Orelli's emendation: *iubenti M. Bibulum in vincula duci*).

The most natural inference from Cicero's statement is that Vatinius had made the necessary preparations and was actually dragging Bibulus to prison, when stopped by his colleagues. Compare Dio. XXXVIII 6 ἐπεχείρησε μὲν γὰρ αὐτὸν ἐπὶ τούτοις Πούπλιος τις Ἀτίνιος δῆμαρχος ἐσ τὸ οἰκημα καταθέσθαι· τῶν δὲ συναρχόντων οἱ ἐναντιωθέντων οὐκ ἐνέβαλεν.

The Scholiast goes on to say: *quod vero ad tabulam Valeriam pertinere videatur, loci nomen sic ferebatur, quemadmodum ad tabulam Sestiam, cuius meminit pro Quinctio, ita et ad tabulam Valeriam dicebatur, ubi Valerius Maximus tabulam rerum ab se in Gallia prospere gestarum proposuerat ostentui vulgo.*

From this statement we infer that *ad tabulam Valeriam* was a definite spot in the city where the tribunes who interfered with the carrying out of Vatinius' design were for some reason gathered.

The Scholiast supports his explanation by referring to the *tabula Sestia* mentioned in the *orat. pro Quinctio* 25, and ascribes the origin of the name to the fact that Valerius Maximus had a painting made representing his deeds of prowess in Gaul. Valerius Maximus, however, won renown in Sicily, not Gaul, and *Gallia* must in any case be an error for *Sicilia*.

Compare now the passage, first cited by Orelli, in Pliny, N. H. XXXV 22, where, after speaking of the painting by Fabius Pictor on the wall of the temple of Salus—a painting which still existed in Pliny's time, though the temple had been burned in Claudius' reign—we read: *dignatio autem praecipua Romae increvit, ut existimo, a M'. Val. Maximo Messala, qui princeps tabulam pictam proeli quo Carthaginienses et Hieronem in Sicilia vicerat proposuit in latere curiae Hostiliae anno ab urbe condita CCCXC (490/264).*

Without doubt the Scholiast drew his information from Pliny,

and there is no reason for discrediting the latter's statement that such a painting had been on the wall of the Curia Hostilia.

Of the history of the Curia Hostilia down to the time of Sulla, tradition is silent. Pliny (N. H. XXXIV 26) says: *invenio et Pythagorae et Alcibiadi in cornibus comitii positas, cum bello Samniti Apollo Pythius iussisset fortissimo Graiae gentis et alteri sapientissimo simulacra celebri loco dedicare. Eae stetere donec Sulla dictator ibi curiam faceret; and Dion Cass. XL 50 ἦν μὲν γὰρ (τὸ βουλευτήριον) τὸ Ὀστῖλιον, μετεσκεύαστο δὲ ὑπὸ τοῦ Σύλλου.*

According to these statements, the old senate house was taken down entirely or restored by the dictator. The Curia which he built was burned not many years later during the Clodian riots of the year 54, and rebuilt by Faustus Sulla, under the name of the Curia Cornelia. Cicero (de Fin. 5. 2) says: *Hostiliam dico, non hanc novam quae minor mihi esse videtur postea quam est maior,* showing that the name Curia Hostilia continued to be applied to the building, after Sulla's restoration, until its destruction in 54.

There is then no doubt that Sulla restored or enlarged and rebuilt the building, and the question at once arises whether a painting on the wall of the old Curia would have been preserved or replaced on the wall of the new.

We know not what vicissitudes the Curia may have undergone between 263 and Sulla's restoration, but in the passage previously quoted from Pliny (XXXV 19) we are told that Fabius' painting on the walls of the temple of Salus was still in existence, although the temple itself had been burned under Claudius. Up to this point the only thing of which we can be certain is that there was on the wall of the old Curia a painting of Valerius' victory. If there was then a *tabula Valeria*—in the sense of a painting on the wall—in 58 B. C. it must have been the original carefully preserved and perhaps transferred to the wall of the new building, or a copy of the original made at the order of Sulla. No proof or disproof of either of these hypotheses is possible, but the latter is perhaps more probable than the former.

The second question then arises: Is there any evidence for the truth of the Scholiast's statement that in the year 58, *ad tabulam Valeriam* was the designation of a definite spot?

Manifestly this is connected with the preceding discussion, to this extent, that if there was such a painting on the wall of the Curia at that time, it would be natural and entirely probable that the spot beside the wall should be called *ad tabulam Valeriam*.

If the picture had been destroyed, the name might have clung to the spot, but in view of the probable changes in the building made by Sulla, it would hardly have done so unless some particular importance was attached thereto. Certain officers might have been stationed there, certain official acts have been performed there, or something of similar nature. Instances of this sort of topographical tradition are common enough everywhere, as the custom of calling small districts after the name of an old tavern, years after every trace of the tavern has disappeared.

But the Scholiast supports his statement by citing Cicero *pro Quinctio* 25, *ad tabulam Sestiam*. The context is as follows. Quinctius and Naevius, after quarrelling over their pecuniary obligations to each other, had separated without giving bonds to appear in court at any particular time. Naevius stayed in Rome, while Quinctius started for Gaul. As soon as Naevius found that Quinctius had got as far away as Vada Volaterrana, he proceeded to do what Cicero describes in the following words: *pueros circum amicos dimittit, ipse suos necessarios ab atriis Liciniis et a fauibus macelli corrogat, ut ad tabulam Sextiam sibi adsint hora secunda postridie. Veniunt frequentes. Testificatur iste P. Quinctium non stitisse et stitisse se; tabulae maxime signis hominum nobilium consignantur, disceditur. Postulat a Burrieno praetore Naevius, ut ex edicto bona possidere liceat.*

Evidently *ad tabulam Sestiam* was a place where formal declarations of a legal sort were properly made, and testimony taken.

We know nothing of any Sestius or Sextius whose deeds may have been commemorated by a painting, nor have we the slightest hint elsewhere of the possible location of this *tabula*, and can therefore add no evidence to this part of the Scholiast's testimony.

Those who maintain that *tabula Valeria* means the 'bank of Valerius' start with this passage in *pro Quinct.*, interpreting *tabula Sextia* as the bank or exchange of some Sextius. These are the only two cases where *tabula* is used in the singular with an adjective derived from a proper name. Cicero uses the word of an auction bill and apparently of an auction room (*ad Att. XII* 40. 4; *XIII* 33. 4; *XV* 3. 2), but nowhere of a banker's table. While there is perhaps nothing in the expression itself which would render such a view impossible, it should be at least clearly required by the context. This is not so here, for there is no reason why, for a legal declaration of this sort, Naevius should

call his friends together at some money-lender's. Applying this meaning of *tabula Sextia* to *tabula Valeria* in the passage in the letter to Terentia, Tyrrell (ad loc.) explains its sense thus: "*Tabula Valeria* is the 'bank of Valerius.' It seems to have been customary in Rome for a person about to make a solemn statement as to his solvency or such like matters to repair to a banker's, and there make the statement in presence of witnesses. It was to make such a solemn declaration that Naevius summoned his friends *ad tabulam Sestiam* (pro Quint. 25). Terentia was probably forced by Clodius to repair to the bank of Valerius, there to make some declaration about her husband's estate, probably that no effects had been made away with, or that she was not keeping the property under the pretence that it was hers. We see from a previous letter that Cicero had resorted to some means to evade the full rigor of the confiscation. It cannot have been merely to borrow money that Terentia was taken to the *tabula Valeria*. There would have been no humiliation, if she had had credit enough to borrow from the bank; and no object in taking her there if she had not."

In criticism of this view, it is to be said that it rests on two pure assumptions, one that *tabula* in these places means bank, and second that such a bank was an ordinary place of legal declaration, with the inference from these premises that Terentia was to swear to some facts about her own or Cicero's property. These may be true, but it is important to bear in mind that they are not yet proven.

The second explanation of the phrase is that suggested by Manutius' conjecture that there was a sort of tribunes' court *ad tabulam Valeriam*, to which Terentia was forced to go by Clodius, presumably to answer for Cicero's property in some way or give security therefor. His explanation was of course based on the expression in the inter. in Vat. 21: *a tabula Valeria collegae tui*, which may well mean that that was the assembling place of the tribunes, and that those who were there assembled prevented Vatinius from thrusting Bibulus into prison. Certain topographical arguments can be adduced in support of this view.

Plutarch in his Life of Cato the Younger (¶5), speaking of the Basilica Porcia, says: *εἰωθότες ἐκεῖ χρηματίζειν οἱ δῆμαρχοι καὶ κύρος τοῖς δίφροις ἐμποδὼν εἶναι δοκοῦντος ἔγνωσαν ὑφελεῖν αὐτὸν η μεταστῆσαι.*

Compare further Cic. pro Sest. 124, where Sestius the tribune

venit, ut scitis, a columnā Maenia; and ib. 18: alter (Gabinius) . . . ne in Scyllaeo illo aeris alieni tamquam fretu ad columnam adhaeresceret, in tribunatus portum perfugerat.

The *columnā Maenia* is probably then the *klōv* mentioned by Plutarch, and stood in front of the Basilica Porcia, which was itself close to the Curia. The painting would be on the side of the Curia rather than on the façade, and it and the *columnā* might be so close together that the station of the tribunes could be designated in either way.

Again in Suetonius, *Iul. Caes.* 78, we read: idque factum eius tanto intolerabilius est visum, quod ipse triumphanti et subsellia tribunicia praetervehenti sibi unum e collegio Pontium Aquilam non assurrexisse adeo indignatus sit, ut . . ., showing that the tribunes sat outside the Basilica, and close to the Sacra Via, the route of the triumph, which ran in front of the Basilica.

Mommsen cites also CIL. VI 2340: *publicus a subsellio tribunorum*, which refers to a public slave attached to the office of the tribunes, showing that such a local station was recognized.

It being granted that the tribunes did assemble near where there had been a painting of Valerius' victory, it must be shown further that this explanation of *ad tabulam Valeriam* gives the desired sense in the two passages in question. This is certainly the case in the passage in *Vat. 21*. *A tabula Valeria collegae tui* means the rest of the tribunes assembled there and either actually witnessing Vatinius' unlawful proceedings or possibly only knowing of his purpose. The former is altogether more likely, and just what we should expect.

Further, the form of the expression is parallel to the inscriptive *publicus a subsellio tribunorum*. The other explanation of *a tabula Valeria* here leaves us with no known reason why Cicero should have used the phrase at all.

Applying this interpretation to the passage in Cicero's letter, Manutius' conjecture is justified by the sense given. Clodius would naturally drag Terentia to his own official station, if he suspected that she was helping her husband to evade confiscation, and might require her to give some security. We are wholly in the dark as to the exact nature of the proceedings, and they may have served no purpose except to humiliate and insult Terentia.

To sum up—there are in support of the view that *ad tabulam Valeriam* denoted a definite spot, so called from the painting,

and in this case the place where the tribunes were wont to assemble—(1) the statement of the Scholiast; (2) the evidence that the tribunes did gather near the Basilica Porcia; (3) the propriety of the expression *a tabula Valeria collegae tui*, and the sense so given to that passage (in Vat. 21); (4) the good sense also given to the passage in the letter to Terentia.

Against this interpretation it may be urged (1) that it is improbable that the painting on the wall of the Curia was preserved, or a new one made, by Sulla, and that therefore there is a still greater improbability that the name remained attached to the spot, even if it had once been given; and (2) that the translation 'bank of Valerius' is the more satisfactory in the passage in the letter to Terentia. In answer to this last objection, even if we grant that this meaning is equally good in relation to Terentia (and the preceding discussion prevents our allowing it to be better), it certainly is not satisfactory in the connection of in Vat. 21.

In regard to the first objection, if we grant at once that it was unlikely or even impossible that the original painting should be preserved, in spite of the case already quoted from Pliny of the painting by Fabius on the temple of Salus, still it is not at all unlikely that it should have been reproduced on the new Curia by the command of the dictator. This picture must have been one of the famous things in Rome, and its origin, associations and connection with the senate house must have made it something which the people would have been loath to lose.

The more we reflect upon its peculiar character, history and surroundings, and the comparative rarity of such things in the early days of the city, the more we shall be convinced that it would be most natural to speak of the adjacent open space as *ad tabulam Valeriam*.

Any argument from the use of *ad tabulam Sestiam* (pro Quint. 25) is without value in support of either view.

SAMUEL BALL PLATNER.

## V.—THE ORIGIN OF THE GERUND AND GERUNDIVE.

"The Origin of the Gerund and Gerundive," "Further Notes on the Origin of the Gerund and Gerundive," and "Concluding Notes on the Origin of the Gerund and Gerundive"—such respectively are the titles of the three papers upon the above-named subject, which through the courtesy of Professor B. L. Gildersleeve have been published as follows in the *American Journal of Philology*:—

- (1) Vol. XV, part 2, July 1894, pp. 194-216;
- (2) Vol. XVI, part 2, July 1895, pp. 217-222;
- (3) Vol. XVIII, part 4, Dec. 1897, pp. 439-452.<sup>1</sup>

In order fitly to close and "unify" the work, thus distributed over three volumes, an Index of some kind has been suggested as, if not indeed indispensable, at least advisable.

A general summary, in precise terms, of my view concerning "the Origin of the Gerund and Gerundive"—in other words: a concise statement of the main propositions which it was the object of my said three papers to establish, together with references to all the material passages in support of each such proposition—will be found in the "Precise Statement of my view (with references)" given towards the close of the third paper (*A. J. P.* XVIII, 1897, p. 449).<sup>2</sup>

It were too much of an encroachment upon the available space of this Journal to give here a reprint of the said summary; to

<sup>1</sup> Addenda et Corrigenda (*italicised* for clearness' sake) as follows:—P. 443, n. 3, insert "441." P. 444, n. 5, read "216, *text ad fin.*" P. 446, text, l. 22, read "pp. 441, 442 and 443." P. 447, text, l. 18, read "representative"; p. 447, n. 3, read "216, *text ad fin.*" P. 449, n. 6, read "441, 442, 443, 445, 446." P. 449, n. 7, read "439, 446, 447 *sqq.*" P. 449, n. 8, read "439, 446 *sqq.*" P. 451, n. 5, read "446 *sqq.*"

<sup>2</sup> With respect to the first of the propositions there enunciated, see further the two Postscripts given at the close of the third paper (*A. J. P.* XVIII, 1897, pp. 450-452).

which, therefore, the present reference must, as no doubt it amply will, suffice.

I will here content myself with giving an Index solely of the various *Word-forms* cited or discussed in the course of the said three papers:—

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[NOTE:—Although the papers are spread over three volumes, the page-numbers in no case clash; consequently, it will be unnecessary to specify in each case the number of the volume referred to. Suffice it to say that pp. 194-216 refer to vol. XV, pp. 217-222 refer to vol. XVI, and pp. 439-452 refer to vol. XVIII.]

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(b) *The Romance Languages.*

a [in, e. g., ho a scrivere (Ital.)] 197. complément (French) 210 n.; compliment (French) 210 n. Future tense: formation of, 211 sq.

(c) *Umbrian.*

anferener 212; asamař 197; aseriatō eest 203 n. ebetrafe 196; ehia-, see 212, 442; ennom enom 222 n.; erom 211 n. fašiu(m) 195, 196; fero(m) 195, 196. kaleřuf calersu 202, 211, 451 n.; kařetu 207; kařitu carsitu 207. manuve 196; meřs 205. pane 195, 222 n., 443; pihaner 212; pone ponne 222 n.; portatu 206 n.; portust 206 n. stiplo(m) 196.

(d) *Oscan.*

aídil 202 n.; A nafríss 448 (and see R. von Planta, *Gramm. der Osk.-Umbr. Dial.*, vol. II, 1897, p. 769). karanter 197 n.;

censaum 196. deded 203. edum 198, 212; eehiia- 212, 442; eehiianasúm 212, 442; ezum 211 n. fatíum 196. húrtín 196. μεδδειξ 205. sakrannas 212. úpsannam 196, 203, 212, 214; upsed 206 n.; uupsens ουπσενσ 206 n. vincter 197 n.

(e) *Marsian.*

atoier 196 n.

E.—GERMANIC.

(a) *Gothic.*

usskáus 218.

(b) *High German.*

scouwōn (Old H.G.) 218. zu (Mod. H.G.), e. g. der zu lobende, 195, 197.

(c) *Modern English.*

complement 210 n.; compliment 210 n.

(d) *Old Icelandic.*

skyn 218.

F.—BALTIC-SLAVONIC.

(a) *Lithuanian.*

geradéjis 207. miřszti 208, 217 n., 221 n. -tinas 194. veizdi (2. s. imperat. act.) 201 n., 440; visagalīs 207; visgalīs 207.

(b) *Old Church Slavonic.*

děla-achū 211. vidě-achū 211.

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## VI.—ON -ΣΣ- AND -Ζ-.

When we compare Homeric words containing -σσ- with their later forms, we notice that Homeric -σσ- corresponds sometimes to later -σ-, sometimes to that -σσ- which is represented in Attic, Boeotian and Cretan by -ττ-. The examples I shall take to illustrate these changes are *τελέσσαι*, *ποσσί*, *πράσσειν*, || *τελέσαι*, *ποσι*, *πράσσειν*. There are three theories which might account for this variation: (i) That the Homeric -σσ- was in one or both of these cases not the historical antecedent of the later form. (ii) That the Homeric -σσ- developed differently in different circumstances. (iii) That the Homeric -σσ- represented two sounds, which developed independently.

Now, as to the first theory, difficult though it may be to establish a historical connexion between the language of Homer and any later dialect, still, few scholars would be willing to deny such a connexion, for without it a large part of Greek Philology would be reduced to guess-work. In the present case some evidence of the connexion can, I think, be adduced. The change from *τελέσσαι*, *ποσσί* to *τελέσαι*, *ποσι*, is phonetically simple when we bear in mind that we have to do, in the first case with I.E. *s+s*, and in the second with *d+s*. Nothing intervenes to break the series *s+s*, long *ss*, short *s*. A comparison of the usages within the Homeric poems themselves also throws light on this subject. Comparing Iliad I–VI (omitting the Catalogue) with Odyssey XXI–XXIV, we find:

	Iliad.	Odyssey.
instances of -σσ- in fut. and aor. forms,	104	72
“ -σσ- in dat. plur.,	142	59
“ -σ- in fut. and aor. forms,	71	79
“ -σ- in dat. plur.,	25	16

Such figures as these cannot, I admit, be pressed to prove very much. Not only does Homer use many forms in -σσ- without any philological justification, but rhythm and set phrases materially affect the numbers; for example, *ἐπεσσί* is rare throughout Epic compared to *ἐπεσσοι* or *ἐπεεσσοι*, while on the other hand the

phrase *σὺν τεύχεσι* almost ousts the form *τεύχεσσι*. Nevertheless, the coexistence of -σσ- and -σ- in early Epic, and the growth of -σ- at the expense of -σσ-, particularly in verb-forms, in the later Epic, seem to establish the historical connexion.

As to the relationship of Homeric *πράσσειν* to the later *πράσσειν*, to admit the descent of the latter from the former drives us to hypothesis (ii) or (iii) in order to explain the variation, while to deny it seems impossible, since there is not the slightest evidence of any difference between the two.

To establish the descent of -ττ- from -σσ- is slightly more difficult, since Attic gives us no hint of any form previous to -ττ-. The earliest Cretan inscriptions, however, do give evidence of the period before -ττ-, and if we may assign the same value and history to Attic -ττ- as to Cretan -ττ-, this gives considerable help. The Cretan forms I shall discuss presently.

As to the second possible theory given above, that the difference between -σσ- and -σ- was due to circumstances of accent, position in the word, etc., it would, I think, be impossible to apply it consistently. There remains the third possibility, that -σσ- in Homer represented more than one sound; what, then, are the sounds that we are to assume to have been expressed by -σσ-? In the first place, I see no other value for the -σσ- of *τελέσσαι*, *ποσσι*, except dental *s*, either doubled or, more probably, lengthened. The other value must be assigned to that Epic -σσ- which is represented in later Ionic by -σσ- and in Attic, etc., by -ττ-, that is, the Epic -σσ- which arises from Ur. Gk. *κ₄*, *χ₄*, *τ₄*, *θ₄*. It is hardly necessary to give examples of this well-known change; but I must emphasize the fact that my view differs from that of Brugmann in supposing a far closer connexion between the dental -σσ- and the guttural -σσ-. Brugmann supposes the former to have been -ss- in prehistoric Greek, and thus makes a form like *μελιττα* difficult to explain; while the guttural -σσ- || -ττ- he takes to be divergent developments from some Ur. Gk. spirant. I think the two sets of forms can be better explained together. We have -σσ-, which is not -ss-, arising from *κ₄* and *τ₄*; the obvious value to assign to -σσ- is ʃ. Both changes are illustrated by the English word *conscientious*, but although the two sounds are now identical in English, the first must have been originally a palatal ʃ, which we may write \*ʃ, while the latter was a supra-dental ʃ ('ʃ). The importance of this difference will appear later.

Contrast with the simple and natural change from  $\kappa_t$ ,  $\tau_t$  to  $\check{s}$ , the series assumed by Meyer (Gr. Gr.<sup>3</sup>, §282),  $tj$ — $tz$ — $ts$ — $ss$ . The first step in this series is unexampled and improbable, since the change in the position of the vocal organs from  $j$  (=  $\check{z}$ ) to  $z$  is no slight one. In the second place, why did not the  $ss$  from this  $ts$  become  $s$  in Attic as it does where  $s$  follows a dental stem? The only way to meet this objection is to suppose that the change  $\tau_t$ — $ss$  was not completed till after dental +  $s$  had become  $s$ ; that is to say, there was a time when  $-\sigma\sigma-$  from  $t\check{z}$  had a different value from that of  $-\sigma\sigma-$  from  $ts$ ; and that period is attested by the Homeric poems. Moreover, how is  $-\sigma\sigma-$  from  $\kappa_t$  to be explained as  $-ss-$ ? The union of  $\kappa_t$  and  $\tau_t$  in  $-\sigma\sigma-$  is to my mind the greatest proof of the existence of  $\check{s}$  as a stage of the development. The next point to discuss is the treatment of this  $\check{s}$  in later Greek. Attic, Boeotian and Cretan treated it in a manner markedly different from dental  $s$ ; they lisped it to  $p$ , which is now commonly regarded as a phonetic approximation to the sound of  $-\tau\tau-$ . Ionic and the other dialects retained the symbol  $-\sigma\sigma-$ , and possibly retained the sound  $\check{s}$ . Smyth (Ion. Dial., §375) hints that  $-\sigma\sigma-$  was not a pure sibilant. The transliteration of  $-\sigma\sigma-$  into Latin as  $x$  (e. g. in *Ulixes, malaxo*) seems to show that Greek  $-\sigma\sigma-$  was not Latin  $-ss-$ , though Greek  $-\sigma-$  was Latin  $-s-$ . Inscriptional evidence is also forthcoming. The sign  $T$  at Halikarnassus and Mesembria interchanges with  $-\sigma\sigma-$  (see Meyer<sup>3</sup>, l. c., note). Now, if  $-\sigma\sigma-$  was pronounced  $-ss-$ , there was no need for another sign; whereas, if  $-\sigma\sigma-$  was not  $-ss-$ , an attempt at a more exact representation was natural; and even if, as some say in order to minimise the importance of the sign, it represented a local pronunciation, why did the provincialism affect only the sibilant which the other Ionians wrote double, not that which they wrote single?

What, then, was the value of Ionic  $-\sigma\sigma-$ ? It was a sound so close to  $s$  that ancient writers give us no hint of any difference, nor has any difference survived in Modern Greek. On the other hand, it was sufficiently unlike to have a different representation, namely the doubled sigma, since doubled dental  $s$  had been reduced to  $-\sigma-$  in Ionic, and to have also a different method of transliteration into Latin. The sound that answers to this description is  $\check{s}$ , and we may conclude that Ionic has remained at the same stage from which Attic, etc., have advanced a step further. Such a conclusion, however, is not likely to go unchal-

lenged. Dr. Blass scouts the idea of the existence of the sound *š* in Greek; he says (Aussprache, p. 92): "Boeckh was inclined to regard this" (such spellings as *εισστην*) "as an indication of the sound *š*, and his suggestion has found many to repeat it; it is, however, as unwarrantable as it is unmaintainable, and is at present given up. The sound *š* is unknown even in cultivated modern Greek: and if the ancients had possessed it, they would doubtless have made use of the proper Phoenician symbol to express it." Against this it may be argued that the phonetic correspondence of modern to ancient Greek is so slight that the fact that the sound *š* has not survived to the 19th century A. D. cannot at all disprove its existence in the 5th century B. C. Secondly, so little is known for certain about the relationship between the Greek sibilant signs and their Phoenician prototypes, that it is difficult to say what Phoenician sign would represent *š* in Greek. Without dwelling unduly on this point, I may give the following sketch of the question, abstracted from Taylor, History of the Alphabet, II, p. 95; Roberts, Greek Epigraphy, p. 8, and Hinrichs, Gr. Epigraphik, in Müller's Handbuch.

The Semitic alphabet possessed the following sibilant signs:

Numerical order.		Sign.	Name.	Value.
i	7	I	Zayin	<i>ds, z</i>
ii	15	׃	Samekh	<i>s</i>
iii	18	Ϻ	Tsade	<i>ts, ss</i>
iv	21	Ϣ	Shin	<i>š</i>

Corresponding to these, we have in the oldest Greek alphabets, the Western alphabets of Caere and Formello:

i	7	I	Zeta (= 3d name above)	<i>ds, z</i>
ii	15	⊕	? (Xi = 4th name above in Eastern)	? (= <i>x</i> in Eastern)
iii	18	Ϻ, M	San (? = 1st name above)	<i>s</i> (?)
iv	21	Ϣ	Sigma (? = 2d name above)	<i>s</i>

The value of the sign I is fixed in nearly all Greek alphabets: the value of  $\oplus$  in the Western group is unknown: perhaps it was merely numerical; in the Eastern group it has the form  $\Xi$  and the value *x*. Hence, if any alphabet were found that used both the remaining signs, it would be natural to assign to one of them the value *s*, and to the other the value *š*. Now at Halikarnassus, Teos and Mesembria the two signs are found in use, and

one of them, in the form T, which is taken to be a variant of M, does represent a sound which on phonetic grounds I take to be š. Professor Ramsay (Jour. Hell. Stud. I) derives the sign from an Asiatic source; but its use at Mesembria, a Megarian colony founded by Chalcedon and Byzantium, makes this less probable, since Asiatic influence could hardly have so wide a range. Other alphabets possessed either M or Ξ only and used them to represent -σ- or -σσ- indiscriminately. The confusion between the signs M and Ξ is shown by the alphabets of Corinth and Metapontum, which put M after P in the place of Ξ.

On early monuments no difference whatever is made between -σ- and -σσ-, but both are written Ξ or M. It was only at a later period that ΞΞ is written, and at all times Ξ appears sporadically instead, while I can find no instance of double M. The adoption of the double sign was, I think, an attempt to distinguish between two closely similar sounds, and the point on which the distinction was based was the fact that the prosodial effect of š, like that of ρ, was that of a double consonant, while σ did not "make position." In early times, then, Ξ represented (i) dental σ, as in λυσαται; (ii) the same sound doubled or lengthened in δικάσσαται; (iii) š in πράσσειν. At a later period the sound of (ii) became short in the dialects of Attica and Ionia, but where in the older literature it made position, it was written double; then, since š likewise made position, that too was written -σσ-. The reason for the prosodial weight of š I shall discuss later.

So much, then, for the sign T with the value š. Turning now to the ancient Cretan inscriptions, given by Comparetti (Mus. Ital. III), I shall endeavour to prove the existence of the sound š there too. In the archaic Cretan inscriptions the sign I represents a sibilant arising from at least four different sources:

- (i) In IOOI and -AIEN, I corresponds to Attic ζ, later Cretan δ-, -δδ-.
- (ii) In OIOΞ, I arises from I.E. *t̪*, and corresponds to Epic -σσ-, Ionic-Attic -σ-, later Cretan -ττ-.
- (iii) In ΑΝΔΑΙΑΘΑΙ (= ἀνδάσσασθαι), I comes from -ts-, and corresponds as in (ii).
- (iv) In FOIHA (= Epic οίκηα), I represents a peculiar archaic Cretan palatalization of κ, unknown elsewhere.

Of these four varieties of I, the last three certainly represent voiceless sounds; the origin of (ii) and (iv) point to a š sound, while the later representation of (ii) and (iii) by -ττ- points to a

sound which was not dental *-ss-*. I consider that the sound of Ι in these three cases was *š*, and that these archaic inscriptions, which are assigned to the 7th century B. C., preserve evidence of a period, unattested by monuments in Attica or Boeotia, before *š* became *p*.

The next question that must be considered is the difference between the guttural *š* and the dental *š*: this difference manifests itself in the passage of dental *š* to dental *s* under certain conditions, whereas the guttural *š* never becomes *s*. Brugmann considers that *-σσ-* from *τι*, *θι* regularly passed to *σ* both after consonants and between vowels, and explains forms like *ἐρέσσω*, *κρέσσων* (Att. *ἐρέττω*, *κρείττων*) as analogical. But how can *οινούττα*, *μέλιττα* be explained on this theory? *ἐρέσσω* may perhaps follow *πράσσω* and *κρείσσων* follow *ἥσσων*, through parallelism of meaning, as Brugmann says. But I cannot see what analogy can retain the large class of feminines in *-εσσα* and *-ασσα*. It cannot be denied that analogy has affected Epic verb-forms in *-σσ-* and *-σ-*; but in such cases analogy is quite as likely to work one way as the other; still, *νεμεσώματι* beside *νεμεσώματι* is the only example of a verb with *-σ-* from *τι* that I can discover, whereas *ἐρέσσω*, *ἰμάσσω* and *λίσσωματι* all show *-σσ-*, and Attic *βλίττω* shows *-ττ-*. Putting aside, then, verbal forms as ambiguous, we have two cases in which *-σσ-* from *τι* becomes *-σ-*, namely: (1) after a nasal consonant; e. g. *τιθείσσα* from *\*τιθεντιά*, *\*τιθενσά*; the presence of a nasal consonant does not affect *-σσ-* from *κι*; e. g. *ἄσσων* from *\*ἀγχιών*. Without the nasal consonant we have, beside Skt. *āpavatī*, Gk. *ὸπεσσα*, for *\*ὸπο-θασσα*, with the strong vocalism of the masculine; so also, beside Skt. *satī*, Gk. *ἴασσα*, for *\*esntiā*, by the side of feminine participles which have the strong stem of the masculine as *ούσα*, *ἴουσα*, for *\*esontiā*. Brugmann (II 400) gives *ἀέκασσα* and perhaps *πρόφρασσα* and *θέρμασσα* as weak feminine participles.

(ii) The second case of the reduction of *-σσ-* from *τι*- *θι*-, to *-σ-* occurs in the three sets of words:

*μέσσος*, Ion.-Att. *μέσος*, from *\*μεθλος*,  
*τόσσος*, *πόσσος*, etc., Ion.-Att. *τόσος*, from *\*τοτλος*,  
*πρόσσω*, *δπίσσω*, Ion.-Att. *πρόσω*, from *\*πρότλω*.

Brugmann notices that in these cases Cretan and Boeotian show *-ττ-*; e. g. Boeot. Cret. *ὸποττος*, Cret. *μέττον*. Accordingly, if Attic *μέσος* is a reduction from Homeric *μέσσος*, the change must be pre-historic, since *-σσ-* is not to be found in Attic, and *-ττ-* could

not have been reduced to *-σ-*. The antiquity of the forms *μέσος*, etc., is attested by their frequency in the Homeric poems, though they are not so common as *μέσος*, etc. In Iliad I-VI (omitting the Catalogue) occur

*μέσος* 6 times, *όσος*, etc., 17 times, *πρόστω*, etc., 9 times;  
*μέσος* 2 times, *όσος*, etc., 5 times, *πρόστω*, etc., 1 time.

Total with *-σσ-*, 32; with *-σ-*, 8. In Odyssey XXI-XXIV occur

*μέσος* 5 times, *όσος*, etc., 17 times, *πρόστω*, etc., 3 times;  
*μέσος* 2 times, *όσος*, etc., 10 times, *πρόστω*, etc., 1 time.

Total with *-σσ-*, 25; with *-σ-*, 13. The proportion of forms with *-σ-* to those with *-σσ-* is thus twice as great at the end of the Odyssey as at the beginning of the Iliad. We can thus see *-σσ-* passing to *-σ-* before our eyes: but why it should do so in these forms and not in *χαρίσσα*, etc., is not clear. The only point of resemblance between the sibilant of *μέσος*, *όσος* and *πρόστω* as opposed to that of *χαρίσσα*, is that the former is preceded by an accented vowel, while the vowel before the latter is unaccented. But even this distinction does not appear in the case of *κρέσσων*, which, since it should be more properly *κρέσσων*, a form which is found in Ionic, might be expected to occur as *\*κρέσων*.

Although the connexion between this position of the accent and the change of 'j to s is not clear, the closer approximation to s of 'j as compared with \*j makes the change less surprizing. We may suppose that the lengthening of the vowel in *τιθέσσα*, due to the absorption of the nasal, obscured the sibilant sound, and perhaps assisted by *τιθείσ* with dental s, led to its passage to s: while the position of the accent in *μέσος* may have had a similar effect. The two cases cannot be considered parallel, since the first was proethnic, whereas the second was not completed till the Homeric period. The difference is well illustrated by Cretan and Boeotian, which show *-σ-* in the first case, but *-ττ-* in the second.

These two dialects show such a curious likeness in their use of the group *-ττ-* that they deserve special mention. They are the only dialect areas outside Attica that show the lisped j, and when we consider that there was no great connexion between them, the similarity of their usages is startling.

In Crete the sound arising from *κτι*, *ττι* is represented in four different manners at four different periods, the latest embracing

the spread of the *κωνή* with its -σσ-. The earliest inscriptions show I, which, as I said above, I take to represent š. The Gortyn inscription shows -ττ- like Attic and Boeotian; but Cretan approaches more closely to Boeotian than to Attic in two points: First, the dental š does not become s in *μέσος*, etc., and second, the group dental + s becomes -ττ-; e. g. Cret. aor. ἐδατταμαν, beside pres. δατηθαι, Boeot. aor. κομιττάμενος. I do not believe that these are cases of assimilation of spirant to stop: *ts* rather became /š—š; the archaic ἀνδαξαθαι preserves this stage; and then š became þ later, like the š from κι, τι. The third stage of Cretan shows θθ for the -ττ- of Gortyn and the archaic I, in θαλαθθας, θθθακιν (Mus. Ital. III, p. 681), Ἀρκαθθι (op. cit., p. 691), the dat. plur. with θθ from δ+s, like ττ in ἐδατταμαν of the Gortyn inscription and I of the archaic ἀνδαξαθαι. If θθ was merely a graphic variation of ττ to represent þ, as I think, this is conclusive against the theory of assimilation of s to t. This third stage shows θθ also for στ in ιθθάντι (Cauer, Del.<sup>1</sup> 42), a change most curiously paralleled in the Boeotian ιττω of Aristophanes, and ἐττε for ἐστε at Orchomenus. J. and T. Baunack, to explain ιθθάντι, assume the stages στ—þτ—þþ; this may be quite correct, since there is no necessity to explain -θθ- from -στ- on the same principle as -θθ- from dental + s; for the regular appearance of -στ- in the Gortyn inscription, e. g. in κατισταμεν, shows that the passage of στ to θθ was much later than that of dental + s to ττ, θθ. It is to be noticed, however, that even in archaic Cretan, σ is assimilated to a following θ.

Dr. Blass has an article in the *Jahrbücher f. Philologie* for 1891, p. 1 seqq., on an inscription from Phaistos in Crete, containing the words ΠΠΑΤΕΙ and ΕΥΓΛΟΘΟΙ (?), which he assigns to the first century B. C., but which Halbherr, who first edited it in *Mus. Ital.* III, p. 559, assigns to the third century B. C. In his article he advances the view that θ was the hard explosive aspirate in Cretan even at this late date, and that ττ in the Gortyn inscriptions was a double stop. He then explains

Gortyn \*\*Ἀρκαττι (analogous to ἐδατταμαν) : later Ἀρκαθθι  
Gortyn πράδδει : later πρατ(τ)ει

by assuming "eine art lautverschiebung," though he admits that this new "Grimm's Law in Greece" does not affect the aspirate θ. If, however, we may believe, on the authority of Meister, the Baunacks, Comparetti, and Dr. Blass himself in his *Aussprache*,

that  $\theta$  was  $\beta$  in Cretan as early as the Gortyn period, these forms can be otherwise explained. At that period Cretan possessed an inter-dental spirant,  $\theta$ , developed from the dental aspirate, and a supra-dental spirant,  $\tau\tau$ , developed from the spirant  $\check{s}$ . At a later period these two sounds were confused, and were both written  $\theta$  or  $\theta\theta$ . Hence  $\piορτιαθθαν$  (Mus. Ital. I, p. 44) =  $\piροσονσαν$  || Gortyn *latta*. If the forms  $\piρατ(τ)ει$ ,  $\epsilonσπρεμιττεν$  = Attic  $\epsilon\kappaπρεμνι\zetaειν$ ,  $\kappa\alphaπολογιττεθθω$ , quoted by Dr. Blass, belong to this period, we must put them alongside the form *Tηνα*, Doric *Δᾶνα*, Attic *Ζῆνα*, and assume that when  $\tau\tau$  ceased to represent  $\beta$ , it was used instead of  $\theta\theta$  to express  $\theta$ .

The history of initial  $\kappa_i$ ,  $\tau_i$  is not so easy to trace. In the first place, the materials are scanty; secondly, the need of expressing the syllable weight of a final short vowel preceding was not felt to be sufficient to justify the use of a doubled initial  $\sigma$ , so that it is difficult to distinguish between *s*,  $'\check{s}$ , and  $''\check{s}$  when initial; thirdly, dentalized gutturals cause further ambiguity; e. g. how can it be determined whether Megarian  $\sigma\acute{a}$  = *τίνα* came from  $*\kappa_i\acute{a}$  or from  $*\tau_i\acute{a}$ ? We have from  $\kappa_i$  or  $\tau_i$  Ionic  $\sigma$ - in *σεύω*; this  $\sigma$  frequently makes position in Homer, and lengthens the augment in every case but one. In this it appears to have the value  $\check{s}$ ; those cases in which a short vowel remains short before *σεύω* may be explained partly by assuming a poetic license, similar to but perhaps not so harsh as that by which *Σκάμανδρος* appears in hexameter verse, partly by supposing that the poet attended occasionally rather to the written form of the word than to its pronunciation. As regards syllable weight, I equate  $\check{s}$  exactly with  $\rho$ , and initial  $\rho$  does not always lengthen a preceding short vowel. Brugmann connects doubtfully with *σεύω* the Attic *τευτάοματ*, *τευτάζω*, explaining the initial  $\tau$ - as a shortening of  $-\tau\tau-$  which would have appeared in the augmented and reduplicated forms. But if initial  $\sigma$  in Ionic *σεύω* was  $\check{s}$ ,—and we can hardly suppose that the sibilant of *εσσενε* was  $\check{s}$ , while that of *ὅτε σεύατο* was *s*—why should not initial  $\tau$  of Attic have had the same value as medial  $-\tau\tau-$ ? Another example from  $\kappa_i$  or  $\tau_i$  is the Megarian  $\sigma\acute{a}$  quoted above, to which corresponds the Attic enclitic *-ττα*, as in *πόστα ττα*, whence, by wrong division, *ἄττα*. If *-ττα* had ever followed a consonant, would it not have been written *-τα*?

From  $\tau_i$  we have Ionic *σήμερον*, *σήτες* (in Etym. Mag.) beside Att. *τήμερον*, *τήτες*. The origin of these forms from the pronominal stem which appears in Skt. as *tya-* would support the pronunci-

ation of initial  $\sigma$ -,  $\tau$ - as  $\check{\sigma}$  and  $\check{\tau}$  but for the fact that there exist many little-understood cases of Attic initial  $\tau$  corresponding to Ionic  $\sigma$ -, where the origin of the two sounds seems to be *ty*; e. g. *τύρβη*, Ionic *σύρβη*, *τηλία* || *σηλία*, *διαττάω* || *σάω*: with these must be grouped the forms *τύ* || *σύ* and *τέτταρες* beside *τέσσαρες*. These forms are quite separate from those discussed in this paper. Whether the -ττ- of *τέτταρες* and *διαττάω* represented a double stop or a spirant, and what was the process of its development from *ty*, I cannot at present determine.

It will be seen that in the theory of -σσ- given above, I have not hesitated to ascribe more than one value to a single Greek sign in one and the same alphabet. The assumption that the Greek alphabet, like most others, did not possess sufficient consonant signs to represent accurately all the sounds of the language which was written in it, is one that has been frequently made by philologists and passed over without notice. The assumption is, I think, quite justifiable, for it would be a miracle if a borrowed alphabet could express all the sounds of the language that borrowed it. The fact that some of the Greek symbols were conventional, e. g. -σσ-, -ττ-, -δδ-, which, it must be remembered, were not distinguished in writing from -σ-, -τ-, -δ-, in early times, is to me less surprising than the fact that the Greeks themselves do not appear to have thought the matter worthy of remark.

This assumption, then, I make in the case of  $\zeta$ , for no one value has ever yet been proposed for this sign which is satisfactory in every case in which the sign appears.

The following points appear to me clear with reference to the pronunciation of  $\zeta$ :—(i) that in *ἴζω*, *Ἄθηναζε*, Boeot. *θεόζερος*, etc., it was *zd*. *ἴζω* preserves I.E. *zd*, while the other two forms are late compounds. (ii) that after the time of Alexander (see Blass, Ausspr., p. 91) it was dental *z* in *ζμύρνα* *ζβέννυμι*, etc. (iii) that in other forms it was neither *zd* nor *z*. Take, to begin with, the Epic-Ionic-Attic  $\zeta$  in *φύζα*, *σχίζα*, *κράζω*, *πεμπάζω*. In these cases  $\zeta$  is manifestly the outcome of I.E. *dʒ*, *gi*, and many attempts have been made to bridge over the gulf between these sound groups and the sounds *zd*, or *z*. Blass (p. 125) assumes the series *dʒ*—*dz*—*zd*, the last change being due either to metathesis or to the analogy of I.E. *zd* in *ἴζω*. Both steps are difficult, for *dz* is a long way from *dʒ*, and the conversion of *dz* to *zd* is almost impossible. G. Meyer (Gr. Gr.<sup>3</sup>, §284), recognizing this, derives *zd* straight from *dʒ*, in defiance of the law “*Natura saltum non*

facit." He justifies the change by appealing to the O.C.S.  $\check{z}d$  from  $d\ddot{i}$ . But (i) O.C.S. is a long way from Greek; (ii)  $\check{z}$  is not  $z$ ; (iii) at best this is only *ignotum per ignotius*. I would explain O.C.S.  $\check{z}d$  by equating the  $\check{z}$  to the  $d\ddot{i}$  and assuming the  $d$  to be parasitic. Hoffman (Die gr. Dialekte, II, p. 512) argues that original  $\zeta = dz$  must have become simple  $z$ ; he obtains the required value  $zd$  by the series  $d\ddot{i}—dz—z—z\ddot{d}—zd$ . He does not say whether all these changes were proethnic; he does not parallel the adventitious  $\ddot{d}$ , whereas in O.C.S.  $\check{z}t$  from  $t\ddot{i}$  does parallel  $\check{z}d$  from  $d\ddot{i}$ , nor does he explain why it passed from spirant to stop, and that, too, after a spirant. If, then, it is only with the greatest of difficulty that  $zd$  can be obtained from  $d\ddot{i}$ , it would seem to be quite impossible to obtain it from  $g\ddot{i}$ . The parallel between  $k\ddot{i}$ ,  $t\ddot{i}$  and  $g\ddot{i}$ ,  $d\ddot{i}$  seems to me to be quite close. Just as the former pair meet at the sound  $\check{s}$ , which passes in certain dialects to  $\check{y}$ , so the latter pair, as I think, meet at the sound  $\check{z}$ , which passes in Doric to  $\ddot{d}$ . Now the place of articulation of the sound  $\check{z}$  is no more fixed than is that of  $\check{s}$ , and, furthermore, it shades off into other spirantal sounds. Modified in the front it becomes  $\ddot{d}$ ; another modification produces  $z$ . At the back of the mouth it produces spirant  $\check{z}$ , the spirant of the German *morgen*; this in its turn can pass into spirant  $y$  or semi-vowel  $\check{i}$ , as it has in the English *yesterday*, Boeotian  $\iota\omega\tau$  =  $\iota\omega\tau$  for  $\epsilon\gamma\omega\tau$ , and Tarentine  $\delta\lambda\tau\omega$  for  $\delta\lambda\gamma\omega\tau$ . Every one of this series of connected sounds has some bearing on the history of the symbol  $\zeta$  in Greek.

I shall first give evidence for the existence of the sound  $\check{z}$  in Greek, and then consider the value of  $\zeta$  in the different dialects. My evidence is drawn partly from transliteration, partly from phonetic considerations.

Early Latin transliterations give but little help, since the symbol  $z$  had become obsolete in the Latin alphabet, at an early period. Latin accordingly had no symbol wherewith to represent the Greek  $\zeta$ , except  $s$ , which we find in *Saguntum*, and the Plautine *sona*, *tarpessita*, *badiuso*, *comissor*, etc. A curious, but probably quite accidental, resemblance to these last two forms is seen in the Tarentine  $\sigma\alpha\lambda\pi\iota\sigma\sigma\omega$ ,  $\phi\pi\alpha\sigma\sigma\omega$ , etc., with  $-\sigma\sigma-$  for Ionic-Attic  $\zeta$ . This gives rise to the supposition that the Tarentines had transformed their  $-\zeta$ -verbs to  $-\sigma\sigma-$  by analogy, like the Attic  $\delta\mu\delta\tau\tau\omega$  and Thessalian  $\epsilon\nu\epsilon\phi\alpha\pi\sigma\sigma\omega\epsilon\tau$ ; but we have one Tarentine verb in  $-\zeta\omega$ , namely  $\alpha\pi\alpha\zeta\omega$ , and by derivation that should have  $-\sigma\sigma-$ .

Without assuming a 'lautverschiebung' in Tarentine, I suggest that here the signs -ζ- and -σσ- had interchanged values, ζ being pronounced ſ, and -σσ- as ž. Such variations, whether due to analogical transference of forms or confusion of alphabetical symbols, certainly point to a closer resemblance between -σσ- and -ζ- than exists between -ss- and -zd. Early Latin can thus give us but little assistance, but some light is thrown on the question by the correspondence of Late Latin *z*, Greek ζ, to classical Latin consonant *i* and *g* before *e*, and *i*, and *d* before *i*. Lindsay (Lat. Lang., p. 49) considers that the sound in these cases was *y*, and that Latin *z* and Gk. ζ were then pronounced *z*. The subsequent history of the sound he gives as follows:

Lat. consonant <i>i</i>	}	→	Low Lat. <i>y</i> (written <i>z</i> )	French <i>j</i> (= ž)
Lat. <i>ge, gi</i>			Italian <i>gi</i> (= dž)	
Lat. <i>di</i>			S. Italian } <i>y</i>	

Spanish } *y*

But the disparity between the sound and the sign in Low Latin, together with the fact that French and Italian have now a sound which is closer to the original Latin than is this postulated Low Latin *y*, makes me think that the following series more closely represents the facts of the case:

Lat. consonant <i>i</i>	}	→	Low Latin ž or dž (written <i>z</i> , or ζ)	French <i>j</i> (= ž)
Lat. <i>ge, gi</i>			Italian <i>gi</i> (= dž)	
Lat. <i>di</i>			S. Italian } <i>y</i>	

Spanish } *y*

So that the people who wrote for Latin *Julia*, Greek Ζούλεια had not such a bad ear for sounds as to write *z* when they meant *y*: they pronounced the name as a modern Frenchman or Italian would pronounce it. Roby (Lat. Gram. I, §195) assigns the value *dž* or *ž* to Late Latin *z* in these cases.

The passage of consonant *i* to *ž* through the stages *i*—*y*—*ž*—*ž* is illustrated by the Sanskrit *hariya*, transliterated from Greek ὥρια (Wackernagel, Alt. Ind. Gramm., pp. 137, 242). This, however, does not support the value *y* for ζ in Ζούλεια, inasmuch as transliteration out of a language is a vastly different thing from transliteration into it. For example, if Greek possessed a sound *ž*, another language, having no *ž*, might transliterate it as *y*; but that *y* would not be written *ž* in Gk., but probably *i*. Greek ζ represents, not Sanskrit *y*, but a palatal consonant, in 'Οζηνη = *Ujjayini* (Blass, Ausspr., p. 128).

The Sanskrit language paid more attention to phonetics than any other that has ever existed; is it likely that it would have represented *zd* or *z* by *y*? The nearest equivalent to *z* in Sanskrit would be *s*; the nearest to *ž* would be palatal *j* or semivowel *y*.

According to Meyer (Gr. Gr.<sup>3</sup>, §226, note) *σξ* is employed on papyri to represent Arabic and Coptic *š*. Granting the difficulty that any Aryan tongue would find in accurately reproducing a Semitic sibilant, still *szd* seems a very weak attempt to represent any kind of a *š* sound: *sž* is at least intelligible.

As inscriptive evidence I may cite the archaic Cretan I, referred to with reference to the value *š*. I sought to prove that in three cases out of four it had that value; in the fourth case it represents a voiced sound, which I take to be the voiced counterpart of *š*, namely *ž*.

From Cyprus come the forms *ἀξαθὸς* and *ξὰ*, where *ξ* represents the spirantized *y*, that is *ʒ*.

The Cyprian *κορξα*, Aeolic *κάρξα*, *ξὰ* = *διά* show *ξ* as a late formation from *δι*; the sound here was probably Eng. *j*, that is *dž*, or perhaps *ž*.

So far I have treated only of *ξ* from I.E. *dž*, *gž*; but *ξ* from I.E. spirant *y*, in *ξυγόν*, *ξίω*, can also be explained as *ž*. Sievers defines the difference between semivowel *ž* and spirant *y* as due to greater friction. Whether that greater friction is produced by narrowing the air-passage or increasing the pressure of the air-current, the same process that produces *ž* from *i* will, if continued, produce *ʒ* from *i*. If we give this value, *ʒ*, to I.E. *y*, its representation by Gk. *ξ*, that is *ž*, no longer needs to be explained by a complicated process such as that given by Meyer, *j—dž—zd*, which obscures the difference between I.E. *ž* and I.E. *y*. The passage from the spirant to the semivowel in other languages is readily paralleled by the English *yesterday*. This distinction between the palatal semivowel *ž* and the palatal spirant *y* (= *ʒ*) is the same as that between the labial semivowel *u* and the labial spirant *θ*.

Just as the voiceless *š* was lisped to *p* (ττ) in Attica, Boeotia and Crete, so the voiced *ž* was lisped to *d* (δδ) throughout the entire range of the Doric dialect; e. g. Laconian *μονσιδδει*, Megarian *μάδδαν*, both in Aristophanes; Cretan (Gortyn) *δικάδδω*, Boeotian *τράπεδδα*; initial *δ* = *d* is seen in Laconian *Δάν*, Cretan (Gortyn) *δώη*, = Boeotian *δάει*, Sicilian *Δάγκλη*. And just as the supra-dental -ττ- was confused in Crete with inter-dental *θ*, so in Elean supra-dental -δδ- was confused with inter-dental *δ* = *d*.

In Aeolic we find  $\zeta$  = Ionic-Attic  $\zeta$  written on inscriptions; it is also attested by grammarians as the sign employed for the sound arising from the late union of  $\delta$  in  $\kappa\alpha\rho\zeta\alpha$ , and  $\zeta\alpha = \delta\alpha$ ; the symbols -σδ- also occur in such forms as  $\mu\epsilon\lambda\iota\sigma\delta\epsilon\omega$ ,  $\Sigma\delta\epsilon\omega$ , given by MSS and grammarians; the first inscriptional evidence for it is on an archaizing monument of imperial times.

Meyer (Gr. Gr., l. c.) explains σδ as due to the fact that  $\zeta$ , formerly  $zd$ , had become  $z$  in the rest of Greece, and that Aeolic, preserving the sound  $zd$ , adopted a new sign to represent it. My objections to this are as follows: (i) I hold that Meyer has not substantiated the value  $zd$  for  $\zeta$  in all cases, especially from  $gi$ . (ii) It remains to be proved that  $\zeta$  was simply  $z$  in the rest of Greece. (iii) On Meyer's own theory  $\zeta$  in Aeolic  $\kappa\alpha\rho\zeta\alpha$ , etc., was  $z$ ; but  $z$  does not arise directly from  $d\dot{z}$ : the stages are  $d\dot{z}$ — $d\check{z}$ — $\check{z}$ ; a further step, and no inconsiderable one, is necessary to arrive at  $z$ . (iv) Although a dialect might adopt a sign which it did not possess, from another dialect, it would hardly discard a sign which it did possess, because another dialect used it with a different value. According to my theory Aeolic  $\zeta$  was  $zd$  or  $\check{z}$  down to quite late times: the spelling σδ was due to confusion of the two values;  $\theta\epsilon\acute{o}\zeta\alpha\omega$  and  $\theta\epsilon\acute{o}\sigma\delta\alpha\omega$  were equivalent, so beside  $\delta\kappa\acute{a}\zeta\epsilon$  arose  $\delta\kappa\acute{a}\sigma\delta\epsilon$ , with  $\sigma\delta = \check{z}$ . What, then, became of the discarded symbol  $\zeta$ ? It may have been employed to represent the affricate  $d\check{z}$  in  $\kappa\alpha\rho\zeta\alpha$ .

The σδ of the Sicilian Doric of Theokritus is probably merely a literary form. The Doric -δδ- seems to have been entirely banished from elevated literature, its place being taken either by the Ionic  $\zeta$  or the Aeolic σδ. That σδ was foreign to Sicilian might be taken for granted, were it not for the Oscan  $\bar{\nu}\omega\mu\sigma\delta\eta\omega$ , which occurs in a Mamertine inscription at Messana, written about 280 B. C. (Conway, Italic Dialects, No. 1). The alphabet is that form of the Ionic alphabet which came into general use in S. Italy. Two conjectures are open: we may suppose that the sound to be represented, namely Oscan intervocalic -s-, corresponded to the value of the Greek  $\zeta$ , but that it was the fashion at that time in Sicily to represent this by -σδ-. Of such a fashion we have no other evidence except the conflicting spelling of the MSS of Theokritus. Secondly, if we suppose that the Oscan -s- corresponded to no value of -ζ-, that was known in S. Italy, the -σδ- would be an isolated attempt to represent Oscan -s-.

the sound in Oscan, on Meister's theory, would be *zd*, which is obviously impossible; while if Aeolic *-σδ-* was merely a graphic variant for  $\zeta = \check{z}$ , we must believe that Oscan *-si-* was pronounced *-zi-*, which is possible without being probable. On the whole it seems best to treat the two as independent.

The question of the value of the symbol  $\zeta$  in Elean is a curiously complicated one. In the inscriptions of the earliest period  $\zeta$  appears for I.E. and Ur. Gk. *d*; apparently *d* had been spirantized to *d̄*, and I explain the use of the symbol  $\zeta$  in this manner:—Just as there was a period before *-σσ-* (σ) became *-ππ-* (π) in Attic, so there was a period before *-ζ-* (ζ) became *-δδ-* (δ̄) in Doric. In Elean the change of sound was not at first accompanied by a change of sign. That is,  $\zeta = \check{z}$  became  $\zeta = \deltā$ , where the origin of the sound was I.E. *di*, *gi* or spirant *y*. Then, when Ur. Gk. *d* became *d̄*, this too was written  $\zeta$ . Unfortunately, the early inscriptions contain no sure example of a representative of I.E. *di*, *gi*, *y*. In my opinion  $\zeta$  would be found in these cases, with the value *d̄*. Inscriptions after the 5th century show the ordinary Doric spelling *-δδ-* for  $\zeta$ , and *δ* for *d̄*; those at the end of the 5th century represent a transition period; the sign is usually *δ*, with rare lapses to the older  $\zeta$ . For the whole question cp. Meister (II, p. 52), from whom I quote the following: "Dass diese beiden spirantischen Laute des eleischen Dialekts, der durch  $\zeta$  ( $\zeta\alpha\mu\sigma = \delta\eta\mu\sigma$ ) und durch *δ*, *δδ* ( $\delta\eta\gamma\sigma = \zeta\eta\gamma\sigma$ ) bezeichnete sich unterschieden, ist für gewiss anzunehmen, denn wären sie zusammengefallen, so würde man sie nicht durch verschiedene gewählte Schreibung auseinander gehalten haben; worin aber der Unterschied bestand lässt sich nicht erkennen." To this I would reply that there may have been a difference of sound, namely, that between inter-dental *d̄* and supra-dental *d̄*, but the difference was never expressed. The difference of sign is chronological: it does not appear in any inscription except those two of the transition period, one of which shows  $\zeta$  once (in  $\zeta\epsilon = \delta\epsilon$ ) with 10 cases of *δ* unaltered, while the other shows one  $\zeta$  with 20 cases of *δ*.

Arkadian shows  $\zeta =$  I.E. *di*, *gi*, *y*; e.g.  $\delta\eta\kappa\zeta\eta\tau\sigma$  (Meist. II 106; Cauer<sup>2</sup>, 457). A difficulty arises from the appearance of  $\zeta =$  I.E. velar *g* in  $\zeta\epsilon\pi\theta\sigma\sigma$  (Strabo, VIII 8. 4 (p. 389); Meyer, Gr. Gr.<sup>3</sup>, p. 266). The stage previous to  $\zeta\epsilon\pi\theta\sigma\sigma$  could not have been  $\delta\epsilon\pi\theta\sigma\sigma$ , since Arkadian preserves Ur. Gr. *δ* unchanged. The only other similar form is  $\zeta\alpha\lambda\omega = \beta\alpha\lambda\omega$ , given by Hesychius without a

locality. The two forms seem to contain a peculiar product of velar  $\text{g}$ : since it is not a case of dentalization, it seems possible that it arose from a spirantizing of  $\gamma$ , seen also in Cyprian, which  $\gamma$  must have appeared beside the regular representatives of  $\text{g}$  as it does in  $\gamma\lambda\epsilon\phi\alpha\tau\omega$  and  $\gamma\epsilon\phi\nu\tau\alpha$ ;  $\delta$  appears in  $\epsilon\sigma\delta\epsilon\lambda\lambda\omega\tau\epsilon\tau\epsilon$  (Collitz, 1222. 49). Under what conditions  $\delta$  and  $\zeta$  appear respectively cannot be determined with such scanty material. The only other view is that Arkadian contained a mixture of dialects.

I have now to treat of the metrical weight of the sounds represented by -σσ-, -ττ- and -ζ-. In the first place it must be remembered that any continuous consonant can, in the Epic dialect, make a metrically long syllable when following a short vowel. A stopped consonant has not this power, except in very rare cases. In these cases, then, the difference between a heavy syllable and a light syllable depended on the difference between continuous and stop consonant. That is, the greater amount of time spent, or breath used, in the production of a continuous consonant made the syllable containing it long as compared with a syllable containing a stop. In the later language, however, the continuous sounds *l*, *m*, *n* and dental *s* were no longer able to give metrical weight; and such combinations of stop and continuous sound as *tr* were treated in the same fashion. The change was, perhaps, not so much a change of pronunciation, although the sibilant of *ɛdikasσa* may have been dwelt upon longer than that of *ɛdikasa*, but was rather due to a change in the feeling of what constituted metrical weight. The other continuous sounds retained their power of making metrical weight even in classical Greek; *p*, *σσ* (= *ʒ*), *ζ* (= *ʒ*), and -ττ- (= *p*) and -δδ- (= *d*) regularly "make position," and the reason is not difficult to see. The production of these sounds needs a more open position of the vocal chords, and consequently involves a greater muscular exertion and consumption of breath than does that of *l*, *m*, *n*, *s*. The sound *ʒ* or *ʒ* has a more open position than any other sound not a vowel. Consequently it produced a heavy syllable in Greek; and its effect in the pronunciation of English is analogous. Compare the long vowel of *please* with that of *pleasure*, *Asia* with *azure*, *mete* beside *measure*.

To the Greek of the 6th century B. C. this difference of syllable weight afforded the most striking contrast between the sounds *s* and *ʒ*. It was the point on which the different alphabetical representation was based. Consequently it is not surprising to find

that as the language decayed, and syllable weight disappeared before the stress accent, the difference between *s* and *z* likewise disappeared, so that no trace of it survives in Modern Greek. But, in my view, the sounds *š* and *ž* lasted at least long enough to account for the spellings *Malaxo* and *Zouλεια*.

In conclusion, I may briefly summarize my position as follows:

(a) The present views on -σσ-, -ζ-, -ττ-, -δδ- are unsatisfactory because—

(i) *τελέσσαι* and *πράσσειν* could not both have been pronounced with dental *s*.

(ii) The series commonly given to show the development of *κι*, etc., in Gk. contain too many phonetical difficulties, and

(iii) they separate *τι* from *κι* and both from *δι*, *γι*, although -σσ- unites the first pair, and the interchange of -σσ- with -ζ- and the correspondence -ττ- || -δδ- unites the voiceless with the voiced series.

(β) Since a new theory is necessary, the values *š*, *ž* suggest themselves as the representatives of Ur. Gk. *κι*, etc., because—

(i) *κι*, *τι* naturally converge to *š*.

*γι*, *δι*, *ζ* (= I.E. *y*) naturally converge to *ž*.

(ii) Archaic Cretan I in *Foiζηα* shows dialectic growth of *š* from palatalized *κ*.

Archaic Cretan I from dental + *s* } shows dialectic growth of  
Gortyn Cretan *ττ* " " } *š* ( $\Rightarrow$  *p*) from *τs*.  
Boeotian *ττ* " " } *ž* ( $\Rightarrow$  *p*) from *τs*.

Cyprian *ζ* in *ἀξαθὸς* shows dialectic growth of *ž* from *ζ*.

Aeolic *ζ* in *καρζα* } shows dialectic growth of *ž* or *dž* from *δι*.  
Cyprian *ζ* in *κορζα* } shows dialectic growth of *ž* or *dž* from *δι*.

(iii) It is quite possible that the Semitic symbols should, when adopted, have the values assigned to them by my theory, and probable that in the sign *T* we have the fourth Semitic symbol with the fourth Semitic value.

(iv) *š*, *ž* and supra-dental *p*, *d* resemble *r* in their phonetic character as in their metrical effect.

(v) Transliteration, where it gives any help at all, favours my theory, especially transliteration into and from the most scientific of all alphabets, the Sanskrit.

## NOTE.

### NOTE ON CICERO, DE SENECTUTE 54 AND 11.

Professor G. L. Hendrickson, in an article in the October number of the American Journal of Philology, on 'Pre-Varronian Literary History,' p. 291, gives a new interpretation to the words assigned to Cato by Cicero, in *De Senectute* 50: *vidi etiam senem Livium qui, cum sex annis ante quam ego natus sum fabulam docuisse Centone Tuditanoque consulibus, usque ad adulescentiam meam processit aetate.* They are to be regarded, he says, not as a mere "didactic digression," like many other passages in the *De Senectute*, but as an intentional emphasizing of the age of Livius for the purpose of overthrowing a popular error given currency by Accius, and formally refuted by Cicero himself in the *Brutus* (72), probably on the authority of Varro.

One other of Cato's digressions, in *De Senectute* 54, seems open to a similar explanation: *Quid de utilitate loquar sterco-randi? Duxi in eo libro quem de rebus rusticis scripsi, de qua doctus Hesiodus ne verbum quidem fecit cum de cultura agri scriberet. At Homerus, qui multis, ut mihi videtur, ante saeculis fuit, Laerlam lenientem desiderium quod capiebat e filio, colementem agrum et eum stercorantem facit.* The words *qui multis, ut mihi videtur, ante saeculis fuit* are in themselves entirely pointless. But the question of seniority as between Homer and Hesiod was an open one among the scholars of that time. Accius put Hesiod first; Varro disagreed with him (Gell. III 11), and it may well be that Cicero in this passage is again intentionally throwing the weight of his authority on the right side, against the error of Accius, as in *De Senectute* 50.

In *De Senectute* 11, Cicero is himself guilty of a mistake. He names the Roman commander of the citadel of Tarentum, at the time of its recapture, by Quintus Fabius (209 B. C.), as Salinator. It is generally agreed, however, on the authority of Livy (24, 20, 13; 27, 25, 3; 27, 34, 7), that the man in question was Marcus Livius Macatus, not Marcus Livius Salinator. The explanation generally offered for this mistake is merely that confusion between

names so similar was an easy matter, especially since they would often be found without the cognomen. In the light of Professor Hendrickson's investigations, it is possible to conjecture the origin of this mistake more definitely. Accius (Cic. Brut. 72) believed that the poet Livius was captured at Tarentum by Quintus Fabius Maximus in 209 B. C. St. Jerome, chron. ad a. 1830 (187 B. C.), gives evidence of having followed this false chronology of Accius, as shown by C. F. Hermann, quoted by Professor Hendrickson (p. 292). St. Jerome's statement reads as follows: *Titus Livius, tragoediarum scriptor clarus habetur qui ob ingenii meritum a Livio Salinatore, cuius liberos erudiebat, libertate donatus est.* Accius therefore believed that Livius the poet had been the slave of some Livius Salinator, and he brings him into connection with Marcus Livius Salinator by fixing the date of his play in 197 B. C., at the *ludi Iuventatis* vowed by Marcus Livius Salinator in 207 B. C. (Cic. Brut. 72; Liv. 36, 36, 6; American Journal of Philology, p. 291). It would have been most natural then for Accius, who has been proved to be wrong as to several points in regard to Livius, to suppose that the Marcus Livius with whom he connected the poet Livius, in later life, was the Marcus Livius who was conspicuous at Tarentum when he was captured there. That Cicero's error may be dependent upon such an error on the part of Accius seems not improbable. He was acquainted with Accius' literary work, and he even knew him personally in his youth (Cic. Brut. 107). Cicero had made the same mistake about Salinator many years before he wrote the *De Senectute* (De Orat. 2, 273). Meantime, in the *Brutus* (72) he had recognized and refuted Accius in regard to the chronology of Livius. That an error in a single name, dependent upon this more serious error in chronology, should re-appear after the latter had been recognized, would merely convict Cicero of carelessness or forgetfulness, such as must be imputed to him at any rate, from some other cause, if not from the one here suggested.

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## REVIEWS AND BOOK NOTICES.

### CHAUCER.

The Complete Works of Geoffrey Chaucer, edited from numerous manuscripts by the Rev. WALTER W. SKEAT, Litt. D., LL. D., M. A. 6 vols. Oxford, At the Clarendon Press, 1894; with supplementary volume containing Chaucerian and Other Pieces, 1897.

Studies in Chaucer: his Life and Writings, by THOMAS R. LOUNSBURY. 3 vols. New York, Harper & Brothers, 1892.

The Student's Chaucer. Edited by Prof. SKEAT, with Introduction and Glossary. 1 vol. Macmillan & Co., 1895.

The "Globe" Chaucer. Edited by ALFRED W. POLLARD, H. FRANK HEATH, MARK H. LIDDELL, W. S. McCORMICK, with Introduction and Glossary. 1 vol. London and New York, Macmillan & Co., 1898.

In addition to his other well-known works illustrative of Chaucer's writings, Prof. Skeat has now placed all students of Chaucer under increased obligations by the completion of the Oxford edition with the publication of the supplementary volume containing "Chaucerian and Other Pieces." Although the other six volumes have been before the public for a few years, it may be well to state here the contents of each volume. Vol. I contains a Life of Chaucer, the Romaunt of the Rose, and the Minor Poems; II, Boethius and Troilus; III, House of Fame, Legend of Good Women, the Astrolabe, and the Sources of the Canterbury Tales; IV, the Canterbury Tales (Text); V, the Canterbury Tales (Notes); VI, Introduction, Glossary, and Indexes. Prof. Skeat has further edited the complete works of Chaucer in one volume, as "The Student's Chaucer," being the text of the Oxford edition. Mr. Pollard, author of the "Chaucer Primer," edited in 1894 the "Eversley" edition of the Canterbury Tales in two volumes, and now, with the assistance of co-laborers, he has also edited in one volume, as the "Globe" edition, the complete works of Chaucer, preceded by a Life and Introduction and followed by a Glossary. In this embarrassment of riches, especially when increased by the three volumes of Prof. Lounsbury's "Studies in Chaucer," which were published six years ago, it is difficult to see what more the student of Chaucer could desire.

Surely there is no excuse now for an ignorance of Chaucer even on the part of that much-addressed personage, the general reader.

The several chapters of Prof. Lounsbury's work are numbered continuously and embrace the following subjects: I, the Life of Chaucer; II, the Chaucer Legend; III, the Text of Chaucer; IV, the Writings of Chaucer; IV, 2, the Romance of the Rose; V, the Learning of Chaucer; VI, the Relations of Chaucer to the English Language and to the Religion of his Time; VII, Chaucer in Literary History; VIII, Chaucer as a Literary Artist. A brief account of each is given in the Introduction. This is undoubtedly the most complete work on Chaucer and his writings that we possess. It is valuable for its account of exploded errors as well as for its statements of recently ascertained facts. A general criticism, however, may be made, that it is too diffuse; it might have been condensed to advantage. As to Prof. Lounsbury's views on some disputed questions, he acknowledges that they are not those generally held by Chaucer scholars, but he has the courage of his convictions and proceeds to defend earnestly his opinions. This is as it should be. In such a work an author should give the reasons for the critical faith that is in him. But I cannot think that Prof. Lounsbury has settled the questions. In regard to the burning one of the Chaucerian authorship, in whole or in part, of the existing version of the "Romaunt of the Rose," we must render the Scotch verdict in his case, *not proven*. I prefer for the present to take the views of Lindner, Kaluza, Kittredge, and Skeat. Prof. Skeat has already replied satisfactorily to some of Prof. Lounsbury's arguments in the Introductions of vols. I and VI, where he discusses the poem. The "Globe" edition prints the "Romaunt of the Rose" last, and the editor, Mr. Liddell, says of it: "All that we can say at present is that A (vv. 1-1705) may be part of the translation Chaucer says he made; that C is also possibly Chaucer's, but this assumption is less likely than the former; that B (vv. 1706-5810) is probably the interpolation of a Northern writer later than Chaucer who made an attempt to join the two parts of the poem A and C, and make a complete translation, but wearied of the task and dropped it at v. 5810." Prof. Lounsbury's view that Chaucer wrote the *whole* of the present version must be rejected. The evidence of language and metre is against him. Moreover, Chaucer could never have made the bungling junction of 1705 and 1706; something is wrong here. Why, too, did Chaucer refrain from translating the portion omitted between 5810 and 5811, or has that portion alone been lost? The last word has not yet been said on this poem.

A part of Prof. Lounsbury's "Chaucer Legend" appeared several years ago in *The Atlantic Monthly* as "Fictitious Lives of Chaucer." It was hardly necessary to occupy so much space in discussing the spurious "Testament of Love," for, with the reference to the "Troilus" before us (Skeat's ed. VII, p. 123, ll. 253-4), it is

hard to see how Chaucer could have written it, even if there were no other arguments against his authorship. The uncritical judgment of earlier editors is responsible for the inclusion in Chaucer's works of many writings now known to be spurious. Prof. Skeat has shown us that Moxon's edition is a prime offender and has been uncritically followed. Both Profs. Lounsbury and Skeat give due credit to Tyrwhitt for his sound judgment in editing Chaucer, but, unfortunately, he lived before the days of the Chaucer Society and the recent investigations of Middle English grammar and versification. In one important point I must take issue with Prof. Lounsbury. He says (Introd., p. xxv): "It will be observed, also, that in most instances the extracts that are introduced from Chaucer's writings appear in our present spelling. The reasons for adopting this course will be found at the end of the seventh chapter" [II 264-279]. I have re-read these reasons, but they do not carry conviction to my mind. Some of Prof. Lounsbury's quotations sound as if they were taken from a very bad text of Chaucer. The question is more one of pronunciation, especially of accent, than of spelling, and Chaucer's spelling is a key to his pronunciation as well as to his grammar, and pronunciation determines the rhythm, without which there can be no complete enjoyment of Chaucer. Modernize the spelling and we destroy the rhythm. The case of Shakspere is different, for his spelling is much nearer that of the present day, it does not affect his grammar and rhythm, and there is no question here of the final *-e*. I think that correct rhythm is an aid "towards the appreciation of the beauty and power of Chaucer's poetry" (III, p. 273), while fully conceding that "the literary study of Chaucer is one thing; the linguistic study is quite another." The former is certainly helped by the latter, even if the latter is very elementary. On this point I concur with Prof. Skeat (V, p. xxv).

Prof. Lounsbury's chapters V, VII and VIII are of particular interest, and chapters V and VII mark a distinct advance in our knowledge, showing the results of careful study of subjects which have never before been so well treated. The modernizations of Chaucer, from Dryden and Pope to Wordsworth and Leigh Hunt, are rightly characterized as failures, but no one is in danger of mistaking these for Chaucer, whereas, if we undertake to modernize Chaucer's language, we produce a Chaucer that is not Chaucer.

There is one reference of Prof. Lounsbury's, repeated four times, which is an oversight. In I, 271, 273, 442, and III, 44, we have reference to Beaumont's letter to Speght of June, 1597, and in each case he is referred to as "the dramatist Beaumont." According to the common chronology, the well-known dramatist, Francis Beaumont, was at that time about thirteen years of age, and could scarcely be referring to "those ancient learned men of our time in Cambridge," who "did first bring you and me in love with him," i. e. Chaucer. The reference is manifestly to Francis

Beaumont, the justice, father of the dramatist, as the dates correspond, and he died in 1598.

Prof. Skeat has based his text of the Canterbury Tales on the Ellesmere MS, but gives at the foot of the page the most important variants of the other MSS. He regards the spelling of the Ellesmere MS as approaching most nearly to that which Chaucer himself used, and in default of Chaucer's own autograph this is as near as we can ever hope to come to it. The arrangement of the Chaucer Society is adopted for the order of the Tales. Mr. Pollard also, in the "Globe" edition, uses the Ellesmere MS as the basis of his text, recording variants in an abridged form, which is explained in the Introduction. He too adopts the Chaucer Society's order of the Tales by groups, which may now be regarded as the standard.

The General Introduction of a hundred pages in Prof. Skeat's volume VI explains fully his objects in producing this excellent edition. He wished to provide "a thoroughly sound text," founded solely on the best MSS and the early printed editions; also to separate Chaucer's genuine works from the spurious, which have not yet been totally suppressed; and again, it was necessary to take advantage of "the recent advances in our knowledge of Middle English grammar and phonetics." He has also provided a very full body of Notes, which furnish needful help in the explanation of Chaucer's allusions. The Glossary too is very full and excludes all non-Chaucerian forms and words. The words in Fragment A of the "Romaunt of the Rose" are included in this general Glossary, while a separate one has been supplied for Fragments B and C, and still another for the "Tale of Gamelyn." Indexes of Proper Names, of Authors Quoted or Referred to, and a list of Books Referred to in the Notes, together with a formidable list of Errata and a General Index, are appended. The Introduction contains a summary of Chaucer's pronunciation, his treatment of open and close *o* and *e*, his peculiarities of rime, metres and forms of verse, grammatical outlines, versification, and remarks on his authorities. We are thus provided with all necessary help for an intelligent appreciation of Chaucer, and have no reason to think that this work will ever be superseded. In his treatment of versification Prof. Skeat assumes that a verse consists of a succession of "speech-waves," each containing a strong syllable, alone, preceded or followed by a weak syllable, or both preceded and followed by such weak syllable, hence, besides the iambus and trochee, he makes much use of the amphibrach. He is very scornful of the "wooden method," which "breaks up the line into bits of equal length," and "exhibits the result as the Procrustean formula to which all lines of five accents should be reduced." After a careful reading of his remarks I do not think that anything is gained by his method. No one who divides each iambic pentameter into five iambi and marks the feet by dividing lines imagines that there

must be a pause at each dividing line, but he would read the line precisely as Prof. Skeat does. Otherwise he would imitate the old-fashioned scanning of the dactylic hexameter, which some of us may have heard in our youth, as *Roma vi—rumque ca—no Tro—jae qui—primus ab—oris*,—English pronunciation too,—but that belonged to the antediluvian period of school-teaching, and has been long since relegated to the abode of departed spirits, some of them very real spirits in their day. Prof. Skeat insists that "there is no elision at the medial pause" (p. xxxi), and that when *his* loses its accent, it loses also its initial *h*. Both statements may well be questioned, especially the last. Unaccented *his* may easily be pronounced *hiz*, not *iz*, which is a species of Cockneyism that Chaucer knew not of. The question of elision is a matter of ear, and to my ear the line flows much more smoothly when final unaccented *e* before a vowel is elided at the pause as well as elsewhere. The hiatus even there is very offensive. So too with respect to the substitution of trochee for iambus. If *whán that* is a trochee in (1), why is not *whan they* in (59) and *which that* in (3385)? Prof. Skeat is himself inconsistent in this respect, for on p. xxxi he writes *háth in* (8) and on p. lxxxix, *hath in*, same line. It all depends upon which of the two words we regard as having the heavier stress, and opinions may differ about that. So, too, elision of *e* may be employed in the terminations *el. en, er*, as in *my fáder* (3385) even at the pause, and synizesis in *with mány a tére* (B 3368), as often in Milton and other later poets. I should also elide the *e* of the termination *-est* in *Thou rávisé(e)st* *doun fró the déítée* (B 1659), and I should prefer to contract *to han* to *t'an* (223), as *to* is often elided before a vowel. Again (p. lxx), *with yén faste y-shette* (B 560), I should elide *e* and count *y* as a separate syllable *contra* Prof. Skeat.

But these are small matters, and in general I should concur in Prof. Skeat's scanning, even if I should make more frequent use of elision and contraction. It is certainly true that "mere counting of syllables will not explain the scansion of English poetry. Accent reigns supreme, and the strong syllables overpower the weak ones, even to the extent of suppressing them altogether" (p. xcvi); hence the greater need of elision and contraction.

The supplementary volume containing "Chaucerian and Other Pieces" is a distinct addition, and a valuable one, to the usual editions of Chaucer. Here we have twenty-nine pieces which, from time to time, have been attributed to Chaucer, or appended to his works, for the earlier editors, even while printing some of these pieces with Chaucer's works, did not assert that they were written by him, and later editors have made this mistake. These pieces are valuable for reference, even if they were not written by Chaucer.

The true authorship of the prose "Testament of Love" has at last been discovered, and the discovery is due to Mr. Henry

Bradley, co-editor with Dr. Murray of the Oxford English Dictionary. Prof. Skeat discovered that the first letters of the chapters of this work formed an acrostic, but he read the last word incorrectly, and so attributed the work to an imaginary "Kitsun." (See note 2, p. xii, vol. V.) Mr. Bradley discovered that the last six leaves of the MS were out of place, and after arranging them in the right order, we have as the acrostic: "Margarete of virtw, have merci on thin Usk." This confirmed Mr. Bradley's previous conjecture that Usk was the author of the work, i. e. one Thomas Usk, who was executed March 4, 1388. As he refers to events that happened towards the end of 1384 or later, the work is dated about 1387. (See Introduction to vol. VII, p. xx.) Prof. Skeat gives us (pp. xxiii, xxiv) some particulars about Usk, who, it seems, had been inclined to Lollard opinions, but recanted. He was, however, executed on the charge of treason by the Duke of Gloucester's party, as he favored the King against the Duke, whose regency the King was trying to overthrow.

I may correct just here some misreferences to this work, caused perhaps by the cancellation of certain pages which were reprinted and the references overlooked. On p. xxii, l. 20, ll. 131, 132 should be ll. 73, 75; l. 29, p. 140 should be p. 123; on p. xxvii, l. 29, p. 140, l. 292 should be p. 123, ll. 256 ff. The text of this work is due to Thynne's edition of Chaucer of 1532, as no MS copy of it has been discovered.

It would fill too much space to enumerate the titles of all these spurious pieces, but we find here one of Gower's, two of Hoccleve's, one of Scogan's, ten of Lydgate's, one of Sir Richard Ros's, one of Henryson's; and such well-known pieces as "The Cuckoo and the Nightingale," attributed to Sir Thomas Clanvowe, "The Flower and the Leaf" and "The Assembly of Ladies," by the same unknown authoress, "The Court of Love"—first printed by Stowe in 1561 from a MS still in existence in the library of Trinity College, and written in a hand of the sixteenth century—and some half-dozen shorter pieces, thus including works of twelve or fourteen different authors, and this does not exhaust the list.

We have here in one volume and in good texts all the most important pieces that have ever been attributed to Chaucer, and some unimportant ones. Prof. Skeat thinks (p. lxxiv) that the only correct method of drawing up a canon of Chaucer's genuine works is that adopted by the late Mr. Henry Bradshaw: "take a clean sheet of paper and enter upon it, first of all, the names of all the pieces that are admittedly genuine; and then see if it can fairly be augmented by adding such pieces as have reasonable evidence in their favor." By such a method he himself proved twenty years ago that "'The Court of Love' has no claim to be considered at all." In fact, such progress has now been made in a knowledge of Chaucer's style, grammar and metre that any

new claimant for admission to the list of genuine works must prove its right by fulfilling the requisite tests. The burden of proof is on that side. No one would now think, as formerly, of attributing works of the fifteenth century to Chaucer, for the language alone would suffice to convict the applicant of a false claim.

While the beautiful Oxford edition may not be within the reach of all, its cost alone sufficing to limit its circulation, the "Student's Chaucer," which contains the same text, and the "Globe Chaucer" are well within reach and will both serve to popularize a knowledge of Chaucer's works. The "Globe" edition is the latest claimant for favor. In it Mr. Pollard has written the "Life of Chaucer" and edited the "Canterbury Tales" and the "Legend of Good Women"; Mr. Heath has edited the "Minor Poems"; Mr. Liddell, the "Boece," "Treatise on the Astrolabe," and "Romaunt of the Rose"; and Mr. McCormick, the "Troilus and Criseyde." Although Prof. Skeat and Mr. Pollard both use the Ellesmere MS as the basis of their texts, we meet with occasional variations; lack of space forbids illustrations.

Prof. Skeat's edition of course "needs no bush"; it speaks for itself; but that of Mr. Pollard and his co-laborers can be cheerfully commended, and its very moderate price will also commend it to a large portion of the reading public.

JAMES M. GARNETT.

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Caesar De Bello Gallico. Books I-VII. According to the text of Emanuel Hoffmann (Vienna, 1890). Edited with Introduction and Notes by ST. GEORGE STOCK. Oxford, At the Clarendon Press, 1898. Pp. xxi, 224, Introduction, + 334, Text and Notes.

This is an imposing volume, calculated to excite great expectations. Have we a new recension of the text? No. The text of Hoffmann, which the editor says he found "prescribed by the University," is closely followed, and no mention is made of the later recensions of Meusel and Kübler. No account is given of the important MSS, or of the classes  $\alpha$  and  $\beta$ . Textual difficulties are often passed over without remark. Occasionally we are told that the best MSS or nearly all the MSS have a certain reading. The summaries given before each book are quite long, that to the seventh taking up fourteen pages. The commentary is brief, often a page of text being accompanied only by a line or two of annotation. Clearly, then, we have neither a complete critical nor a complete exegetical edition. We may congratulate the author upon the knowledge which he himself has gained in the preparation of the work, but why he was selected for the task is

not clear, as his previous studies seem to have lain along entirely different lines. Fortunately, he had friends, as we learn from the preface, who were able to lend him various works bearing upon Caesar, some of them of great value; but he seems to have made no serious effort himself to discover what has been done for Caesar in recent years. Thus he mentions his indebtedness to Eichert's Caesar-Dictionary, but seems not to have heard of either Merguet's or Meusel's complete Lexica. He has great admiration for Napoleon, but does not refer in the preface to Colonel Stoffel. He mentions D'Arbois de Jubainville, but does not seem to know of Holder's *Alt-keltischer Sprachschatz*, although de Jubainville refers to it in his preface to *Les Noms gaulois chez César*. Other omissions might be noted, but we do not propose to give here a complete Caesar bibliography. Although "the main object of his book is to treat Caesar as an historian," he does not refer to any of the German essays which treat of Caesar's 'Glaubwürdigkeit.' Indeed, outside of text-editions, the only German works he mentions are Marquardt's *Staats-Verwaltung* and the Caesar-Dictionary of Eichert. Is this to be set down to insular prejudice? The introduction is discursive, pleasantly written, and not without value. Its seven chapters deal with the Commentaries, character of Caesar, Wars with the Gauls (treated at great length, pp. 34-84), Gaul, Britain, Germany, and the Roman Army. Great originality will not be found in the treatment, unless we count as such statements like this, that "for aught we know to the contrary, the epitomes of the lost books of Livy may have been composed by Livy himself." We are told that the birth of Dio Cassius is put about 155, "just two centuries after Caesar's first landing in Britain," but from 55 B. C. to 155 A. D. does not make *just* two centuries. To the scattered notices given about Tanusius should be added the fact that he is mentioned by Strabo XVII 829, according to the best MS. Florus' date can be approximately fixed by his preface, so that it is misleading to say that "his date is quite unknown, except that he mentions Trajan." Instead of speaking of Ariminum being *founded* in 268 B. C. it would be better to say *colonized* by the Romans. A close kinship of the Ligurians with the Gauls still remains to be proved. The abrogation of Caepio's command was not followed immediately by exile, as one would infer from the statement on p. 77. The name Albion probably goes back at least to Pytheas. In a note on p. 167 we find "Cp. the fragment of Sallust (assigned by Cortius to the sixth book of the Histories) which is quoted by Isodorus (sic). *Germani inectum renonibus corpus tegunt.*" Why should Cortius (1724) be mentioned here, rather than Maurenbrecher (1891), who assigns the fragment to the third book? *Tarruntenus* on p. 179 is more correctly written *Tarrutenius*, cf. *Prosopographia Imperii Romani*, pars III, p. 296. In an amusing excursus on p. 317 devoted to a visit to Bibracte (Mont Beuvray), the editor with engaging frankness tells us that

he had never heard of Eumenius until he went to Autun, where he was told that he was an orator who lived about 310 A. D. He afterwards noticed in Autun a street Rue Eumene, and Smith's Dictionary confirmed for him the date of the Panegyric on Constantine. It would not be difficult to show that there are many other things of which the editor has never heard. Commenting on 1. 46 he says of *per fidem*, "treacherously. The phrase '*per fidem*,' which originally meant 'by reliance on,' is here on its way to the sense expressed by perfidy." The views of Usener, Stolz and Lindsay are thus completely ignored. On *malacia* 3. 15 we are told: "This appears to be the only passage either in a Greek or Latin author in which the word is used in this sense"; but compare *Acta Apostolorum Apocrypha* (Leipzig, 1891), I, p. 50: "in Hadria autem malacia habita in nave, Theon Petro ostendens malacia," etc.; see *Archiv*, VII 586 and several other articles in the *Archiv* where the word is discussed. Occasionally rather elementary syntactical points are explained, yet the notes on the whole are brief, sober and sensible. Much attention is paid to the identification of sites and to the explanation of proper names, but there is only one map to illustrate Caesar's campaigns and one plate to make clear Caesar's bridge, so that in illustrative material it is inferior to many existing editions. The book, despite its size, does not mark any decided advance in the treatment of Caesar.

M. WARREN.

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Vocabularium Iurisprudentiae Romanae, editum iussu Instituti Savigniani. Vol. I inchoaverunt OTTO GRADENWITZ, BERNARDUS KUEBLER, ERNESTUS THEODORUS SCHULZE, continuaverunt BERNARDUS KUEBLER et RUDOLFUS HELM. Fasciculus II. *accipio-amitto*. Berlin, Georg Reimer, 1898.

The present fascicule of this important work follows after an interval of four years the first, which was reviewed in this Journal, vol. XVI, p. 377. In the interval two of the editors of the first fascicule, Schulze and Gradenwitz, have retired from the undertaking, although some of the articles now published were prepared by them before their retirement; but the editors of the present fascicule are Kuebler and Helm. The general plan of the work remains the same as stated in the previous notice. Each page has two columns with fifty-three numbered lines, so that cross-references are easily found. The present number contains columns 97 to 416, but the number of words embraced is only about 375, of which about 75 are only found once, so that 300 words take up more than 300 columns. *Actio* alone, however, which is of course one of the most common legal terms, occupies coll. 103-131, while the preposition *ad* takes up coll. 134-192. *Alius* requires 25 columns, *alter* 16, *aliquis* 14 and *ago* 11.

It is as interesting to note the absence or rarity of some words as the frequency of others. *Advocatus* does not occur as frequently as one might expect, *amanuensis* occurs but once, *amator* but once, while *adulter* and *adulterium* take up a couple of columns. Of words not included in Harpers' Dictionary we have noted only the following: *adiectamentum*, *adnego* (? for *abnego*), *aliquilibet*. Most compounds of *ad* are given in the unassimilated form, even though the assimilated form is the one which occurs exclusively or commonly in the texts under consideration. This is usually stated. Consequently some words are included in this fascicule which are to be found in Harpers' under *ass-* or *att-*. Especially noteworthy is the frequency of *alioquin*, while *alioqui* seems to be attested for only three passages in the Digests. The list of words found only once is too long to give here, but among them are *accumbo*, *acetabulum*, *addubito*, *adinvenio*, *aemulatio*, *agmen*, *albesco*, *alimonia*, *aliquotiens* and *amburo*.

*Acervus* is used with the genitives *frumenti*, *pecuniae* and *stercoris*, and the passages quoted are not noted in the lexical article on *acervus* in the Archiv, X 280. The article upon *actio* is of course much more complete upon the legal side than that in the Archiv, IX 116 ff.

The various uses of *ad* are carefully classified, and one is struck with the frequency with which it occurs with verbal substantives in *-io* and with gerunds and gerundives, the latter preponderating. Once we find *ad praestandam quinque operarum praestationem*. To be noted is the fact that *admodum* in the sense of *valde* follows its adjective or adverb except in two passages, whereas in Cicero it more frequently precedes. *Adsentio* occurs three times, *adsentior* four, no striking preference being shown for the deponent form. The construction preferred with *adscendo* is *in* and the accusative, only with *Capitolium* the *in* is more frequently omitted. The impersonal use of *adsoleo* is also in legal language the more frequent, although it is used twice personally. Mention is made of *aer* in only four passages, for it is *naturali iure omnium communis*. The absence of frequentatives and diminutives is noticeable. *Actito* occurs but once, and *aedicula*, *alicula* and *agellus* once each. *Ambo* has in the accusative both *ambos* and *ambo*, while *ambobus* in dat. pl. is used twice for the feminine. For *alias . . . alias = modo . . . modo* a few more passages are given than are indicated by Wölfflin, Archiv, II 237. *Ait* is used not only with a personal subject, but also with *oratio* and *lex*. It is also combined with *ita*, *sic*, *quemadmodum*, *sicut* and *ut*. Besides *ais*, *ait* and *aiunt*, the only other forms occurring are *aiebam*, *aiebat* and *aiebant*.

The whole work is expected to be completed in fifteen fasciculi, and a third fascicule will probably be issued in 1899. The classical philologist who wishes to compare juristic usage will find the work most helpful, and we can only hope that it will be brought to a conclusion as soon as possible.

M. WARREN.

**Yale Studies in English.** ALBERT S. COOK, Editor. II. Aelfric.  
 A New Study of His Life and Writings. CAROLINE LOUISA  
 WHITE, Ph. D. Boston, New York and London, Lamson,  
 Wolffe & Co., 1898.

This study is based upon Dietrich's investigations, published in 1855 and 1856, in Niedner's *Zeitschrift für historische Theologie*, and its original purpose was to render the most important part of Dietrich's work accessible to English readers. In works where we should look for some reference to Dietrich, he is wholly ignored. Dietrich's papers considered Aelfric's writings; the teachings of the O. E. church according to Aelfric's writings; Aelfric's education and character; and Aelfric's life.

Dr. White has subjected Dietrich's work to a most careful examination, and to independent investigations, which have confirmed, in great part, Dietrich's results, and enriched them with the fruits of the last fifty years' study in this field. The 'New Study' is introduced by a chapter on the monastic revival, not found in Dietrich. In succeeding chapters we have accounts of Aelfric at Winchester; at the abbey of Cernal; and at the abbey at Eynsham, in which a result is reached different from that of Dietrich, according to whom Aelfric returned from Cernal to Winchester, and perhaps went elsewhere. The chapters on Aelfric's education and character are translations from Dietrich, as is also that upon the exploded theories of his identity, but with modifications and additions. The chapters upon Aelfric's writings include original and independent reviews of his Homilies; Grammatical and Astronomical Writings; Lives of the Saints; Pastoral Letters or Canons; Translations from the Bible; On the Old and New Testaments; Life of Aethelwold; *De Consuetudine Monachorum*; and Prefaces. In Appendixes Dr. White gives us More's Treatise on Aelfric's Identity; Summaries of Förster's Study of the Sources of the Exegetical Homilies; Reum's Study of the Authorship of the *De Temporibus*; McLean's and Tessmann's Studies of the O. E. Interrogations; Ott's Study of the Sources of the Legendary Homilies in Lives of the Saints, I; and Assmann's Study of Aelfric's Judith. A very full Bibliography, a Classified Bibliography, and an Index, make Dr. White's Study a complete apparatus for the student of Aelfric, brought down to date, in which all that is valuable and ascertained in past scholarship is preserved and made accessible. Great charm and vividness is given to the treatment by the biographical and historical details and portraiture. The recovery of so engaging a personality in O. E. literature and life from the confusion in which it became obscured, gives a romantic interest to the study.

No attempt is made to give the views of the O. E. church drawn by Dietrich from Aelfric's writings. This is wisely left to separate treatment, which should include a comparative study of the writings of others, for a satisfactory revision of the

antiquated views of Lingard and Soames. As nearly all of Aelfric's writings before unpublished have been printed since Dietrich's papers appeared, the scholar is now better prepared to undertake it.

There remains much work upon the sources of the Exegetical Homilies, to which Max Förster has made valuable contributions in the *Anglia*, in order to make them serviceable in the criticism of Aelfric's Biblical translation.

Miss White's 'New Study' is a solid and admirable piece of work, which does honor to American O. E. scholarship, and especially to the work at Yale University. It is a revelation of the great advance among us in recent years in O. E. studies, which promises to overtake at no distant day the German scholarship, which has hitherto held the first place.

CHARLES EDWARD HART.

## REPORTS.

### PHILOLOGUS, LII.

I, pp. 1-12. A. Dieterich: Die Göttin Mise. The cult of this chthonic goddess (Herond. I 51) spread from Phrygia by way of the islands to Athens, where it was included in the Orphic and Eleusinian mysteries; thence to Alexandria, and Rome, where it was associated with the cult of Bona Dea.

II, pp. 13-37. C. v. Jan: Die Harmonie der Sphären. The coincidence that there were seven great heavenly bodies and seven degrees of the scale led to the Pythagorean doctrine of the music of the spheres. (1) Saturn being highest in space, was supposed to make the lowest tone, as on the lyre the most elevated string made the lowest tone: the moon being lowest, made the highest tone. (2) The Alexandrian theory used by Aratus reversed this: the higher the body, the faster the motion, and so the higher the tone. (3) There was in the period after Christ, another system of fixed tones, probably to be referred to Ptolemaeus.

III, pp. 38-48. L. Bornemann: Pindar's erste pythische Ode ein Sieger- und Todtenlied; cf. id., Philol. XLV 596 ff., on the seventh Nemean. In Pyth. XI the victor's father is dead; in Nem. VII it is the victor.

P. 48. C. Haeberlin: Xen. Hiero VIII 5—an *ἀπὸ κοινοῦ* construction.

IV, pp. 49-57. J. Pantazidis: Verbesserungsversuche zu Euripides' Iphigenia in Aulis.

V, pp. 58-117. C. v. Holzinger: Aristoteles' und Herakleides' lakonische und kretische Politien. As H. derived his *Ἀθηναῖον πολιτεία* from A. (Philol. L 436 ff.), so also his Lakonian, Kretan and probably all his 43 constitutions are to be taken as direct excerpts from A.

P. 117. M. Petschenig emends Ammianus. See also pp. 218, 317, 421, 495.

VI, pp. 118-31. W. Schmid: Noch einmal Kratippos. Notwithstanding the recent defence (Philol. L 32 ff.) of Stahl's view (De Cratippo historico), Kratippos is to be taken as a contemporary of Thukydides.

P. 131. J. Miller: Aristoteles über die Demokratie. Pol. III 10, 1286a, 24 ff. does not contradict Athen. Pol., ch. 41. 2: the

latter, therefore, need not be considered to militate against the genuineness of the writing.

VII, pp. 132-7. J. Sommerbrodt: Ueber den Lucian-Codex der Marcusbibliothek 436  $\Psi$ . The codex practically agrees with Cod. Vind. 123 B, and so belongs to the class of better MSS.

VIII, pp. 138-59, 332-47. H. Düntzer: Catull und Horaz. A literary comparison. Horace reached the highest development of his poetic gift, whereas Catullus manifests some crudities of passionate youth. Catullus may arouse us more, but it was the maturer odes of Horace which reached the highest point of Roman lyric, as in satire and epistle he is unsurpassed.

P. 159. H. Deiter: Zu Cicero's Academica prior. II.

IX, pp. 160-200. O. Crusius: Zu neuentdeckten antiken Musikresten. I. Additional remarks on the Seikilos inscription with accompanying facsimile, showing the notes above the text. II. Fragment of a score of Euripides' Orestes (vs. 330 fig.) with modern vocal and instrumental notation. The chief value of these fragments is in the general conclusions to be drawn touching the value of the tradition about ancient music.

Miscellen, pp. 201-8.—1, pp. 201-2. O. Crusius: Victorinus und Lampridius von Antiochien. According to Phot. bibl. cod. 101, p. 86 Bk., V. (flor. 460 A. D.) is son of L. (flor. 430 A. D.).

2, pp. 202-4. O. Crusius: Das Epigram des Aesop (P. L. Gr. II, p. 64 Bgk.). The verses are from the life of Aesop, which can be traced to pre-Attic times.

3, pp. 204-5. R. Herzog:  $\Delta\epsilon\nu\nu\nu$  (zu Archil., fr. 65); cf. Herond. VII 103.

4, pp. 205-6. J. Lunak: Zu Dictys, IV 2.

5, pp. 206-8. K. Tümpel: Dia die Kentaurin. Aeolis-Thessalia is the common home of the Kentaur-mythus so closely connected with the Aeolian religion.

X, pp. 209-18. F. Hauser: Hyakinthos. Apollo's love for H. can be traced in monuments to the fifth century B. C.

XI, pp. 219-47. P. Viereck: Die ägyptische Steuereinschätzungs-Commission in römischer Zeit. Based on papyri in the Berlin Museum, "Gr. Urkunden, Band I." The commission consisted of the  $\sigmaτρατηγός$  and  $βασιλικός γραμματεύς$ ; in addition, in the city the  $γραμματεύς μητροπόλεως$  and in the country the  $κωμογραμματεύς$ . Tax declarations,  $ἀπογραφαί$ , were made before this committee.

P. 247. O. Crusius: Nachträgliches zum Wiener Euripides-papyrus. Results of a re-examination of the fragments.

XII, pp. 248-94. Th. Zielinski: Verrina. Discussion of various chronological, antiquarian and legal points. (1) The chronology of the processes. (2) The inheritance of Minucius.

(3) On the origin of the quaestio-courts. *Verr. II 15 iudicio sociali* = after the pattern of the procedure in the case of foreigners, confirming Momms., *Röm. Gesch. II* 108. (4) The *edicta repentina* (*Verr. III* 36-8) belong to 73 B. C. (5) The letter of L. Metellus to the province could hardly have been written before Sept. 71, nor much later. (6) Attempts to make uniform the quaestorian map of Sicily. (7) *Crimen navale* (*Verr. V* 110). (8) On the list of Sicilian quaestors: B. C. 73, M. Postumius (case of Heraclius); 72, P. Caeretius (imprisonment of the pirates); 71, P. Vettius (*Crimen navale*). (9) The suit of C. Servilius to be judged like that of P. Quintius.

P. 294. H. Deiter emends *Cic. ad Attic. I* 16. 13.

XIII, pp. 295-317. S. Bruck: Ueber die Organization der Athenischen Heliastengerichte im 4. Jahrh. v. Chr. I. Introduction to the Heliastic court. A little bronze or beechen tablet served for certificate; a citizen after his thirtieth year was eligible for life; attendance, optional. II (pp. 395-421). According to Arist., *Athen. Pol.*, c. 63. 4, the judges of each phyle were divided into ten sections lettered from A to K.

XIV, pp. 318-24. A. Rzach: Zu den Sibyllinischen Orakeln, contains 8 conjectures.

P. 324. J. Lunak emends *Porphy. on Hor. ad Pis. 19, volum* for *scutum*.

XV, pp. 325-31. G. M. Sakorraphos: *Scholia graeca inedita* in *Euripidis Hecubam*, from two MSS of the Library at Athens.

XVI, pp. 332-47 continues VIII, pp. 138-59.

P. 347. J. Lunak emends *Cic., Cato Maior 15. 51, impendium* for *imperium*.

XVII, pp. 348-65. M. Petschenig: Bemerkungen zum Texte der *scriptores historiae Augustae*, contains emendations and remarks on certain transmitted readings, spellings and idioms.

XVIII, pp. 366-79. W. Schmid: Zur Geschichte des griechischen Alphabets. I.  $\Phi$   $\Psi$  in the eastern and western groups. II. Theories of ancients regarding the *litterae priscae* of the Gr. alphabet.

P. 379. O. Crusius shows that Pronektos was erroneously said to have been a Phoenician colony, *Steph. Byz.*, p. 536 M.

Miscellen, pp. 380-84.—6, pp. 380-81. G. Schepss gives emendations to *Boethius de Consolatione*.

7, pp. 382-4. H. Lewy: Philologische Streifzüge in den Talmud. (1) *Schol. Aristoph. Plut. 1054 καιρός τις* refers to the Jewish Feast of Tabernacles. (2) Egyptian beer, *zythus*, was not brewed with salt, but with *ἄλιμος*, a species of salt-wort. (3) Oinomaos of Gadara lived ca. 130 A. D.

XIX, pp. 385-94. L. Voltz: Die *εἰδη* des daktylischen Hexameters, a contribution to the history of Gr. metric. The four original purely euphonic *εἰδη* are *τραχύς*, *μαλακοειδής*, *κακόφωνος* and *λογοειδής*.

XX, pp. 394-421, a continuation of XIII, pp. 295-317.

XXI, pp. 422-30. J. Ilberg: Zur Ueberlieferungsgeschichte des Hippokrates.

XXII, pp. 431-4. G. Helmreich: Galeni *περὶ τῶν ἑαυτῷ δοκούντων* fragmenta inedita, text of Parisinus 2332.

XXIII, pp. 435-41. G. M. Sakorraphos: Observationes criticae ad Aeschinis orationes.

P. 441. C. E. Gleye has three critical remarks on the Historia Augusta.

XXIV, pp. 442-83. O. Seeck: Studien zu Synesios. I. The historic meaning of the Osiris-myth. Aurelian, cos. 400 = Osiris; Caesarius, cos. 397 = Typhus. II. Chronology of the letters.

XXV, pp. 484-8. R. Ellis: Coniectanea in poetas latinas. (1) In Epicedion Drusi. (2) Ad Gratii Cynegetica.

P. 488. O. Crusius interprets Petron. 56.

XXVI, pp. 489-95. E. Ströbel: Die Handschriften zu Ciceros Rede pro Flacco. Includes a new collation of Vaticanus 25.

XXVII, pp. 496-505. M. Kiderlin: Zum zweiten Buche von Quintilian's Institutio Oratoria, has eleven conjectures.

XXVIII, pp. 506-13. J. W. Beck: Die Quellen in den grammatischen Büchern des Plinius Secundus.

P. 513. R. Hartstein believes the spirit of Antinoos' reply, Odyss. XXI 288, is inconsistent with the taunts of the suitors in books XVII, XVIII and XX.

XXIX, pp. 514-22. O. Crusius: Antiquarische Randbemerkungen. (1) On some ancient missiles (bronze discus, etc.). (2) Furnishing of ancient shops (cf. Herond. VII). The sales-room had shelves (*πυργίδες*); the work-room was in the rear.

XXX, pp. 523-33. K. Tümpel: 'Αλκίνοος ἀπόλογος (Od. XI). History of the title from Plato, Aristotle, Plutarch down. Additional remarks by O. Crusius, pp. 533-5.

XXXI, pp. 536-52. M. Manitius: Beiträge zur Geschichte römischer Dichter in Mittelalter (cf. Philol. LI, p. 704). This article deals with Lucretius, Statius, Aemilius Macer, and Terentius.

Miscellen.—8, pp. 553-6. L. Mendelssohn: Zum griechischen Lexikon, discusses *ἐμπροίκιος* and *ἐπιστήμη*.

9, pp. 557-8. L. Erhard: Der Auszug der Cimberni bei Strabo, II, p. 102, suggests *ἀλεθρίαν* for *οὐκ ἀθρόαν*.

10, p. 559. L. Traube defends MSS *exitare* in Catullus, XVII 23 ff.

11, p. 560. G. Helmreich emends Aurel. Victor., c. 76.

12, pp. 560-63. G. Schepss gives readings for Boethius' *Opuscula Porphyriana*.

13, pp. 563-4. M. Krascheninnikoff suggests *ab re natum* for *arrenatum*, C. I. L. III, p. 950.

14, pp. 564-7. U. Wilken comments on the *κατ' οἰκίαν ἀπογραφαῖ* (census-returns) in the tax-registers of Arsinoe, distinguishing them from the entries concerning ownership.

15, pp. 567-72. H. Lewy: Philologische Streifzüge in den Talmud. (4) The proverb "princeps legibus solutus est" is older than Dio Cassius (LIII 18): it occurs in the Jerusalem Talmud, Rasch haschana I 3, of the first century A. D. (5) Mischna Sanhedrin 60 b. shows that the ancient custom of casting up heaps of stones in honor of Hermes *ἐνόδιος* was in Babylonia transferred to Mercurius. (6) Mischna Abhoda zara III 4 shows that the statues of the gods in the baths were for ornament merely. (7) *'Αδριανὰ κεράμια* refers to pottery from the Adriatic Sea, not to the emperor.

16, pp. 573-6. J. Miller considers the actual basis of the Harmodios and Aristogeiton legends to be that through some misunderstanding the deed which occurred at the Panathenaic festival only partially attained the desired end. Hipparchus was killed, Harmodios slain by the body-guard, and Aristogeiton tortured to death on the rack.

17, p. 576. W. Drexler holds that Sarapis himself is addressed as *Νειλαγώγος* in Insc. Gr. Sic. et It. 1028.

XXXII, pp. 577-83. Leo Bloch: Zur Geschichte des Meterkultes, continues Dieterich's article, Philol. LII, p. 1 ff., and discusses the introduction of the Magna Mater worship into Rome.

P. 583. W. Drexler has a supplementary note to Philol. LII, p. 3, on Mismos and Mida.

XXXIII, pp. 584-92. Fr. Hanssen on Il. IX 13-28 declares the verses to be a bit of pre-Homeric poetry, once a part of a lay on the subject of the *Mήνις*—verses which were so well known that they were introduced into both B and I. The argument is based largely on metrical grounds that the verses strikingly resemble the hexameters *κατ' ἐνόπλιον* of the form *dds dds* discussed in Philol. LI, pp. 231-46.

XXXIV, pp. 593-9. R. Peppmüller: Zwei Hesiodea, retains *μίθους* in Op. et D., vs. 263, and suggests other restorations than Nicole's for the lacunae in the Naville papyrus, vs. 169 ff.

XXXV, pp. 600-15. Fr. Reuss: Zu Lysias, gives some of the more important results of the collation of Cod. Pal. X 88 by the late C. A. Pertz.

P. 615. C. Haebel: Zu Aischylos, Pers. 836-7, reads for  $\ddot{\alpha}\lambda\gamma$  either  $\pi\acute{a}la\alpha$  or  $\eta\delta\eta$ .

XXXVI, pp. 616-51. C. Lange: Thukydides und die Parteien. The aim of the article is to strengthen our faith in the greatness of Thuk. He was in his social views a thorough-going aristocrat; in his politics, he regarded a constitution consisting of a wise mixture of democratic and aristocratic elements as best suited to the needs of Athens; but his historical studies and his native insight made it clear to him that great personalities were superior to constitutions and that among statesmen Perikles was without a peer. Yet it was in foreign politics alone that Thukydides fully embraced his ideas, but he heartily recognized in other relations as well the greatness of Perikles' point of view and the purity of his motives.

XXXVII, pp. 652-63. F. Rudolph: Zu den Quellen des Aelian und Athenaios. A reply to Cohn's criticism (*Philol. Anz.* XVI, 1886, pp. 96-103) of the writer's dissertation (*Leipz. Studien*, VII, 1884, pp. 1-137). The writer is still of his former opinion that Athenaios copied Favorinus chapter by chapter, whereas Diogenes used him for compilation, checking by Favorinus, the latter being Athenaios' prime authority.

P. 663. R. Hartstein. The oath by "board and hearth" in *Odyssey*, XIV 158 ff., XVII 156 ff., XIX 303 ff. was made in the presence of the objects mentioned; not so in XX 230 ff.

XXXVIII, pp. 664-702. W. Soltau: *Die annalistischen Quellen in Livius' IV. und V. Dekade*, contains very interesting tables.

XXXIX, pp. 703-14. O. Crusius: 'Kyrene' unter Dämonen. The figure of a female bearing a stalk of silphium and surrounded by three male and four female 'demons,' as seen on a dish unearthed at Naukratis, is taken to represent Kyrene, with the three personified phylae and four colony-cities at Pentapolis.

19, pp. 719-22. E. Hiller: Zu Pindar, Ol. 1. Critical notes on vs. 12, 24, 50, 89, 105.

20, pp. 722-5. L. Cohn: Zu den Quellen des Aelians und Athenaios. Rejoinder to Rudolph, v. supra, pp. 652-63.

21, pp. 725-6. R. Ellis: *Ad Lucan. IX 777-80*.

22, pp. 726-8. E. Ströbel: Zu Cicero's *Academica posteriora*. Discussion of some readings.

23, pp. 728-30. C. Weyman: Novatian und Seneca über den Frühtrunk. Nov. cib. iud. 6 compared with Sen. Epist. 122. 6.

24, p. 730. W. Drexler: Das Bild des Pan von Panopolis, is really the Egyptian Chem, who was a divinity associated with the moon.

25, pp. 731-2. W. Drexler: Die Epiphanie des Pan. Epigr. 1014, I. Gr. Sic. et Ital., is addressed to Pan *συρικτής*.

26, pp. 733-5. H. Lewy: Philologische Streifzüge in den Talmud. (8) *Κράτησις*, a festival.

27, p. 736. H. Nöldeke: *Taīnōs τις* (suppl. to LI, p. 739 ff.) is a name for Bedouin.

28, p. 736. O. Hirschfeld: Arrenatum?, suppl. to p. 563.

GEORGE DWIGHT KELLOGG.

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Pp. 1-36. Kritische und exegetische Bemerkungen zu Philo. II. P. Wendland.

Pp. 37-65. Quellenstudien zu Ciceros Büchern *de natura deorum*, *de divinatione*, *de fato*. R. Hoyer.

Pp. 66-97. *Satura Tulliana*. O. Plasberg. Textual notes on the *De Re Publica*, the *Timaeus*, and the *Paradoxa*.

Pp. 98-120. Der Tod des Kleitos. R. Schubert.

Pp. 121-36. Zu Ciceros Briefen an Atticus. C. F. W. Müller. Textual notes.

Pp. 137-58. Drei boiotische Eigennamen (mit einer Beigabe *Ναΐκραρος ναύκλαρος ναύκληρος*). F. Solmsen. I. *Φιθάδας*. II. *Φάρμαχος*. *Βράμις*.

Miscellen.—Pp. 159-60. R. Kunze. De Strabonis loco. Conjectures *πολύδικον* for *πολιτικόν* in XVII, p. 797 Cas.—P. 160. K. Kalbfleisch. Zum *Anonymous med. Paris.* (Rh. Mus. XLIX 551 f.).—Pp. 160-65. A. Körte. T. Lucretius Carus bei Diogenes von Oinoanda? Rejects the identification of the *θαυμάστος Κάπος* of the inscription with the Roman poet.—P. 165. Fr. Vollmer. Zum *Homerus Latinus*.—Pp. 165-6. M. Ihm. *Damasus und Dracontius*.—Pp. 166-7. F. Buecheler. *Spartiaticus*.—Pp. 167-8. O. Rossbach. *ΗΔΥΣ· ΘΡΑΙΚΙΔΗΣ*. Textual note on Plin. N. H. XXXIII 156.—P. 168. A. Bauer. *Κέπουλε*.

Pp. 169-204. Die 'Hundekrankheit' (*κύων*) der Pandareos-töchter und andere mythische Krankheiten. W. H. Roscher. A refutation of the opinion of W. Kroll (vol. LII, p. 342; A. J. P. XVIII 488).

Pp. 205-8. Oskisches aus Pompeji. F. Bücheler. Discussion of an inscription published in the *Notizie degli Scavi*, Nov. 1897, p. 465.

Pp. 209-38. Studien zu Ciceros Briefen an Atticus (XI-XVI). O. E. Schmidt. Textual notes.

Pp. 239-69. Der 'alte Tempel' und das Hekatomedon auf der Akropolis zu Athen. G. Körte. It is probable that Dörpfeld's "ancient temple" was a double temple (in which Erechtheus was worshipped as well as Athena), and that it had no *θυσιθόδομος*. The name *Παρθενών* may mean the chamber of the *Παρθένος*, not, as Furtwängler has suggested, the chamber of the *παρθένοι*. In an excursus, the writer quotes and discusses the "Hekatomedon" inscription.

Pp. 270-82. Textkritisches zu lateinischen Dichtern. J. Ziehen. Notes on a number of passages in the Latin Anthology.

Pp. 283-307. Bacchylides' Gedicht auf Pytheas von Aigina. F. Blass. A comparison of this poem with Pindar's ode on the same subject, the fifth Nemean. Textual notes and commentary.

Pp. 308-15. Der Thukydides-Papyrus von Oxyrhynchus. J. Steup. This fragment (IV 36, 2-41, 1), which may have been written in the first or second century A. D., furnishes very few new readings of importance. The best of them is the *σταδιαία* of 38, 5.

Miscellen.—Pp. 316-18. C. Weyman. Varia.—Pp. 318-22. O. Hense. Zu Bakchylides XI.—Pp. 322-4. J. M. Stahl. Zu Bakchylides. Textual notes on V 107 ff.; XVII 90 f.; IX 30 ff.; XVIII 31 ff.—Pp. 324-7. F. Rühl. Die Abfassungszeit von Theophrasts Charakteren.—P. 327. Ed. Wölfflin. Pisanders Athla des Heracles.—P. 328. E. F. Bischoff. Epigraphisch-Kalendarisches.

Pp. 329-80. Göttliche Synonyme. H. Usener. A supplement to section XVII of the author's book on the Names of the Gods. To certain heroes mythology has assigned two fathers—the one divine, the other mortal. In such cases we may regularly assume that the name of the mortal father is an older local name of the god. Very often the form of the name is enough to suggest that it was originally applied to one of the numerous conceptions which were afterwards combined under the name of Zeus. For example, Hellen is sometimes called the son of Zeus, sometimes the son of Deucalion. If Heracles is sometimes called the son of Amphitryon, sometimes the son of Zeus, we may assume that Amphitryon was originally the god of the lightning, who sends forth the thunder-bolt "in both directions" (to the east and to the west) and "pierces through" with it. Tyndareos, the father (*ἐπικλησιν*) of the Dioscuri, Castor and Pollux, was the

Spartan god of the lightning, "the shatterer"; cf. Skr. *tud*, Lat. *tundere*, etc. The father of the Theban Dioscuri, Zethos and Amphion, sometimes bears the transparent name of Epopeus. The father of Peirithoos is sometimes given as Ixion, sometimes as Zeus. But Ixion, the lord of the Sun-wheel, is Zeus, as the name of his wife, Dia, might suggest. Aiolos, too, is a synonym of Zeus; cf. *aiόλλη*, Od. XX 27. In Pindar, Ol. IX 42, Zeus is called *Αἰολοβρόντας*. This is a "dvandva-compound," like the *κεραυνοβρόντης*, of Ar. Pax, 376, or the *βροντησικέραυνος*, of Ar. Nub. 265, and means "the god of lightning and thunder." Poseidon, also, is a god of many names. He appears as Glaukos, Aigeus, Neleus, as Hippotes, Hippokoon, Hippomenes, Amphidamas, Aktor and Elatos, as Kretheus, as Aphareus, as Aloeus, as Melanthos, etc.

Pp. 380-92. Zur Datirung einiger athenischer Archonten. Joh. E. Kirchner. I. Damasias. II. Urios. III. Sosistratos. IV. Pheidostratos. V. Andreas. VI. Herodes. VII. Apolexis. Lysandros. Lysandros Sohn des Apolexis. VIII. Architimos.

Pp. 393-8. Das sogenannte Fragment Hygins. M. Manitius.

Pp. 399-431. Der Kalender im Ptolemäerreich. Max L. Strack.

Pp. 432-47. Ueber den Mynascodex der griechischen Kriegsschriftsteller in der Pariser Nationalbibliothek. H. Schöne.

Pp. 448-59. Neue platonische Forschungen. Zweites Stück. Fr. Susemihl. 5. Die Darstellung der Erkenntnisslehre des Protagoras in Platons Theaetatos.

Pp. 460-76. Das *ἐγκώμιον εἰς Πτολεμαῖον* und die Zeitgeschichte. H. v. Prott. I. Der Kult der *θεοὶ Σωτῆρες*. II. Die Familienverhältnisse. III. Die Afsassungszeit des Gedichtes.

Pp. 477-81. Noch ein Wort zur Topographie Korkyras. B. Schmidt.

Miscellen.—Pp. 482-4. A. de Mess. Coniectanea A. Meinekii inedita.—P. 485. Fr. Susemihl. Zu Aristoteles Meteorologie, I i.—Pp. 485-91. U. Köhler. Ueber eine Stelle in der Politik des Aristoteles. A comparison of V iii. 3 Schneid. with Plutarch, Praecepta reg. reip. 32, 825 B.—Pp. 491-3. U. Köhler. Ein Fragment des Demetrios von Phaleron.—Pp. 493-5. F. Malchin. Posidoniania.—Pp. 495-6. M. Ihm. Zu Suetons Caesares.—P. 496. R. Fuchs. *ἀρμοῖ* und *ἀρμῆ*.

Pp. 497-510. Euripides und die Mantik. L. Radermacher. Sometimes the poet's utterances represent the popular sentiment of his day, sometimes his own political views.

Pp. 511-25. Zum ersten Buch des Velleius Paternulus. F. Schöll. Textual notes.

Pp. 526-40. Neue platonische Forschungen. Fr. Susemihl.  
Concluded from p. 459.

Pp. 540-46. Ovid. *trist.* IV 10, 43 s. K. P. Schulze. It is  
probable that the 44th verse refers to a single poem of Aemilius  
Macer, a poem imitated from the *Theriaca* of Nikander.

Pp. 547-74. Zur Handschriftenkunde und Geschichte der  
Philologie. V. Eine griechische Handschrift in Russisch-Polen  
und das Anthologion des Orion. R. Foerster.

Pp. 575-84. Apuleiana. W. Kroll. Textual notes.

Pp. 585-95. Fälschungen in den Abschriften der Herculane-  
sischen Rollen. W. Crönert.

Pp. 596-620. Caeles Vibenna und Mastarna. F. Münzer.

Pp. 621-5. Stilpon. O. Apelt.

Miscellen.—Pp. 626-8. Fr. Susemihl. Die Lebenszeit des  
Eudoxos von Knidos.—Pp. 628-9. E. Goebel. *Ad Gellium*  
(XIX 1, §§2 and 21).—Pp. 629-30. O. Rossbach. Die Olympi-  
schen Solymer.—Pp. 630-33. K. Brugmann. *Ἐπασσύτερος*.—  
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Epigraphica.—Pp. 638-9. A. v. Domaszewski. Der Staats-  
streich des Septimius Severus.—Pp. 639-40. W. H. Roscher.  
Berichtigungen und Nachträge zu S. 169 ff.

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WILFRED P. MUSTARD.

### BRIEF MENTION.

ARTHUR PALMER, whose death has made so sensible a void among the classical scholars of our kin over the sea, was not permitted to finish his work on the *Heroides of Ovid* (Oxford, At the Clarendon Press; New York, The Macmillan Co.). The end of the XIVth Epistle had been reached and most of the commentary for XV, XVI, XVII had been written when he relinquished the task, which, at his urgent request, Mr. L. C. PURSER, the Ciceronian scholar, consented to finish. Though Ovid has been called, unjustly in my judgment, 'an inferior Cicero in verse' (A. J. P. IV 209), Professor PURSER demurred on the ground that he had not devoted any special study to Ovid, so that his part of the edition has been done under a quasi-protest. It is interesting to know that PALMER had learned to believe in the Ovidian authorship of the Epistle of Sappho to Phaon (XV) and interesting to study Mr. PURSER's loyal execution of his friend's commission. Of the five last Epistles PALMER held, and held strongly, that they were not written by Ovid, that they were all, with the exception of XVI 39-142, XXI 13 ad fin., written by the same author, and that he lived about the epoch of Petronius or Persius. The general Introduction is an attractive essay and shows, despite Mr. PURSER's demurrer, that he has a juster appreciation of Ovid than some special students of Latin elegiac poetry. The account of the chief MSS is taken chiefly from Sedlmayer, and the section on Planudes is indebted to what Mr. PURSER styles the "admirable treatise" of Professor GUDEMAN. It is unnecessary to say that PALMER's own commentary abounds in fine touches and enhances the regret that a critic and a scholar of such judgment and such insight should have been removed prematurely from the work for which he had such rare endowments. The Greek version of the *Heroides* by Planudes is a welcome addition to the Latin original. In his doctoral dissertation already referred to, Professor GUDEMAN attempted to determine the character of the codex used by Planudes and in some passages bettered the text, VI 47 *Dodonide* for *Tritonide* being a noteworthy instance. But there are many problems left, and the Planudean version suggests a number of questions as to the tradition of translation from Greek into Latin. It is a pity that Planudes was so poor a Latin scholar, but he must have had some school-training, and the 'flatness and baldness' of his trans-

lation, like the flatness and baldness of the Septuagint, are not to be regretted from a comparative any more than from a critical point of view. In the history of translation from Latin into Greek—a history of more importance than much speculation about the original meaning of this and that case and this and that mood—Planudes, late as he is, cannot be neglected.

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Goethe has a good word to say for the old-fashioned compilations of 'Elegant Extracts'; classical scholars owe much to ancient anthologies, and I have often yearned for something in the range of Greek literature that should correspond to the 'Old South Leaflets.' To preach about Greek literature and Greek style without illustrations is more or less futile, and the laborious perusal of such a work as Schmid's *Atticismus* would be of less service to the average student than the brisk exposition of half a dozen carefully selected pages of Dion Chrysostomos, Lucian, Philostratos or Aelian. In his *Latin Literature of the Empire* (Harper & Brothers), Professor GUDEMAN has undertaken to supply a like need that every teacher of Latin must have felt, and the first volume, containing the Prose Literature from Velleius to Boethius, is before the public. The notes are critical merely and the introductions are brief. The lecturer will have all the scope he desires. Of course, everybody will not be satisfied with Professor GUDEMAN's limitations. Those who are interested in Christian literature might prefer to have something of Tertullian and Augustin, even if Tacitus, so easily accessible, had to be curtailed and the Ciceronianisms of Minucius Felix reduced to a smaller compass. But we must be thankful for what we have, and make the best use of it as a help to lectures on the literature of the period represented. The student who should attempt to handle the book without a guide might be puzzled. So, to go no farther than the first few pages, in the extract from Seneca Rhetor, p. 5, 24, the novice will find himself forsaken by the critical apparatus. Then the extracts from Velleius skip from Caesar (Julius) to Caesar (Augustus), from Marbod to Tiberius, and the death of Cleopatra is put in the year after the battle at Philippi—the kind of thing the unhappy student once had to contend with in Smith's History of Greece. Comp. e. g. Smith, c. XXVIII, §5 (Felton's ed.) with Grote, VI 329. In the extracts from Curtius the dative *Clito* appears as the English form of Clitus in the headline (p. 49). A slip of another order is the use of 'controversial' for 'controverted,' which must be laid to the printer's charge, as Professor GUDEMAN is a native American. Not so easily corrected by the novice are mistakes in the Latin text, and a misplaced comma (p. 34, 34) may bring him to a dead halt. A sharp revision seems to be needed.

In his beautiful edition of *Velleius* (Clarendon Press), Professor ROBINSON ELLIS has made the *Amerbachii Apographon* the basis of his critical work, and has espoused the cause of the young scholar to whom we owe the copy with an affectionate zeal that recalls his enthusiastic characteristic of that youthful genius, Dionysius Salvagnius, to whom he has reared a monument in his edition of the *Ibis*. There is no scholar of our day to whom the erudition of the olden time is so present a reality, as it would be hard to name a scholar who is so much like the men whom he admires in first-hand knowledge.

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Scarcely had DÖRPFELD'S great work on the Greek theatre given us a *point de repère*, when new excavations demanded a revision just as imperatively as the onlooker's ground has to be shifted when the diggers are at work. No wonder, then, that Mr. HAIGH, whose book on *The Attic Theatre* came out in 1889, has found a new edition necessary in 1898. The chapters dealing with the theatre and the scenery have been entirely rewritten, and the chapter on the dramatic contests at Athens has been rewritten in parts. While HAIGH now concedes that Dörpfeld has proved conclusively that the stone theatre at Athens was not earlier than the fourth century B. C., he holds with the grip of a Kynaigeiros to the 'old theory,' admitting only that the stage of the fifth century was much lower than that of later times. The new Haigh is nearly a fourth larger than the old, which falls into the limbo of antiquated things. And even as I write, Professor FOSSUM wheels in his *εισκύκλημα* (A. J. Archaeology, 1898), the grooves for which he thinks he has discovered in the Eretrian theatre, and, if this is so, another section has to be rewritten. Happy is the man who is not committed to any theory on the subject, who is not forced to say with Kleon: *κυλίνδετ' είσω τόνδε τὸν δυσδαιμόνα*.

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In a recent number of the *Rheinisches Museum* (LIV 1), the well-known Thukydidean scholar, J. M. STAHL, who is not averse to airing his grammatical lore, as he has shown by his *Quaestiones Grammaticae*, complains that the German grammars of Greek take no account of the familiar phenomenon of the predicative participle with the translation of an abstract noun. I have no especial interest in the shortcomings of German grammars of Greek, and the fashionable, desiccated school grammar is not the place where I should look for points of style, so that I have not taken the time to verify Professor STAHL'S statements; but I cannot refrain from expressing my surprise that so mature a

student of Greek as he is should say that the only poetic example known to him is Ar. Nub. 1241: *Ζεύς γελοῖος ὄμυμενος τοῖς εἰδόσιν*, which he renders 'der Schwur beim Zeus ist den Wissenden lächerlich.' The construction goes back to Homer (see A. J. P. XIII 258), e. g. Il. 14, 504: *ἀνδρὶ φίλῳ ἐλθόντι γανύσσεται*, and 13, 35, where Monro translates *νοστήσαντα ἄνακτα* 'the return of the master.' Pindar delights in the construction, for which see my Introductory Essay (cxiii), though that passage was written before I had learned that the frequency of the construction in Latin is a frequency in certain authors only (see A. J. P., l. c.). True, the translation by the abstract noun destroys the concreteness of the participle, and Lysias 1, 8: *πάντων τῶν κακῶν ἀποθανόντα (sc. ἡ μήτηρ) αἴτια μοι γεγένηται* the participle is much more plastic than the abstract noun would have been; but that is a matter that really transcends the province of the school grammar.

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I have frequently had occasion to animadvert on the slender attainments of the average German classical scholar in the matter of English, and as some persons have thought that I have thereby done injustice to the learned confraternity of Teutondom, I take from the November (1898) number of the *Berliner Zeitschrift für Gymnasialwesen* the following passage, in a review by H. ZIEMER of the German translation of LINDSAY'S *Latin Language*: "(Es) beherrschen in Deutschland die klassischen Philologen die englische Sprache nicht in dem Grade wie die englischen mit der deutschen vertraut sind." Some years ago Gustav Meyer, in the *Berliner Wochenschrift* (July 27, 1895), said: "Unsere klassischen Philologen lesen noch immer nicht Englisch mit der wünschenswerten Geläufigkeit." And other unsuspected witnesses might be produced to this unwelcome fact—especially unwelcome to American scholars, many of whom look to Germany as to a court of appeal. To be judged by those who do not understand, or only half understand, the language of the pleaders is not exactly the treatment one would expect to receive in the republic of letters. Of course, it will be urged that the best things, like Grote, like Jebb, like Lindsay, compel translation, and that enterprising scholars see to it that their lucubrations are translated into German when they are not composed in German; but there are those who are not willing to yield the primacy of the English language, and, as I have suggested before (A. J. P. XV 398), a return to Latin seems to be one of the possibilities. The Americano-Hungarian *Sermo Latinus* has found an echo in the Roman *Vox Urbis*, and these signs of the times are not to be disregarded. But perhaps English-speaking Hellenists will prefer to follow Wecklein's example, and the *Ζωγράφειος Ἑλληνικὴ Βιβλιοθήκη* will furnish a pattern to classical scholars of all nationalities. *Hoc Ithacus velit*, and the Ithacan is the one enduring Greek type.

The Editor of this Journal never intended to monopolize *Brief Mention*, and he has been trying for some time to make arrangements for the extension of this department and for bringing about a variety in its tone and in its themes. *Brief Mention* ought to be a file-closer, not a stop-gap, nor yet an Editor's Table for the display of Hippokleidean arts or Hippokleidean unconcern. In this number a beginning has been made, and a projected notice of WAIT's *Lysias* is gladly abandoned in favor of a contribution by a new hand.

E. L. G.: Dr. WILLIAM H. WAIT, who has put forth an annotated edition of *Ten Selected Orations of Lysias* (American Book Co.), intends to have his notes meet the needs of young beginners, especially those in the first year at college. "Hence," he says, his "grammatical notes may seem rather full," but the notes consist in great part of references to the grammars of Hadley and Goodwin, and many important and interesting phenomena pass unnoticed, such as  $\omega\sigma$  after  $\phi\eta\sigma\iota\sigma$ , VII 19 (see A. J. P. XVI 396); as  $\omega\sigma$  final, XXXVIII 14, the only example in Lysias (A. J. P. IV 419, note). At the same time, the student in his freshman year is supposed to be familiar with such works as "Kühner-Blass," "Meist." (Meisterhans), "Müller, Handbuch," "Lobeck, Phryn.," and a score of others. Becker's Charicles is cited by the English edition with the page and volume of the German. References to Gilbert's Constitutional Antiquities are sometimes to the English, sometimes to the German edition. A large part of the notes terminate in an interrogation point, an irritating trick, which ought to be, if it is not, out of date. The text is not free from typographical blunders. The notes are far worse. Scarcely a page is clean. The editor's hand is hardly familiar with the useful, not to say indispensable, art of accentuation. One trouble, and a great trouble, is that the notes follow a different reading from that which appears in the text. XII 28 we have  $\bar{a}v$  in the notes and  $\acute{e}a\bar{u}$  in the text; 72,  $\acute{a}tei\lambda\bar{o}\bar{t}\bar{o}$  in the notes,  $\delta i\alpha\pi e\lambda\bar{o}\bar{t}\bar{o}$  in the text. XII 48,  $\pi\lambda\alpha\sigma\theta\acute{e}\bar{v}\bar{r}\bar{a}$  is said to come from  $\pi\lambda\acute{a}\bar{z}\bar{w}$ . Omitted  $\acute{e}or\bar{t}\bar{a}$ 's are carefully supplied, but that does not make up for the omission of the numbers in the headlines, which increases the difficulty of following the references. XXXII 26, 'fifty talents' should be 'twenty-four minae.' XVI 16,  $\acute{a}ya\pi\eta\bar{r}\bar{w}\bar{o}$  is translated with a 'feeling of pleasure,' and not 'barely,' which the student will find with the reference in Liddell and Scott. The map puts Cynoscephalae in Epirus and Coronea in Thessaly, and in spite of his own Index the editor seems to suppose the  $\Pi\acute{o}\bar{n}\bar{t}\bar{o}$  of Satyrus (XVI 4; cf. XXII 14) to be the same as the Pontus of Asia Minor. In short, the editor has not learned the lesson that nothing requires more mature scholarship or riper judgment than the preparation of an edition for beginners.

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M. W.: That after eight years a new edition of the first part of MARTIN SCHANZ's *Geschichte der römischen Litteratur*, in

*Müller's Handbuch*, should be demanded, is not surprising, but it is nevertheless a severe test to the patience of the subscriber to have the concluding part of the history deferred for a year or more by the necessity of this revision. Schanz himself apologizes therefor. The new edition shows an increase of over one hundred pages, and is not only amplified but in many respects improved, although it follows the general lines laid down in the first edition, in the disposition of the material and the attempt to give a general idea of the content of the various literary works and the results of modern discussions about them. The sections on Plautus and Terence and the drama in general, on Cicero, Caesar, Lucretius and Catullus show considerable changes. The new literature relative to the republican period has been most carefully reviewed, which does not mean that all new views and theories have been accepted. Schanz preserves everywhere his independence of judgment. He still follows Drumann's opinion of Cicero, and is not much affected by the attitude of Aly and Zielinski. Leo's new theories on Plautus and Marx's views as to the *Libri ad Herennium* get scant recognition. The Saturnian verse is still for Schanz quantitative, the dramatic *satura*, 'Bockscherz,' is not banished out of existence, and there are other evidences of conservatism. A little more care might be taken with the English names. Peace is printed for Pease on p. 89 and Cosh for M'Cosh on p. 49. Why the latter's edition of the *Bacchides* (1896), the most unscholarly edition of any Plautine play which has appeared in recent times, should be mentioned at all, we hardly understand. It evinces, at any rate, the editor's desire not to ignore recent literature. The new revision must find its place in the library of every Latin professor who wishes to be 'up to date.' An *Alphabetisches Register* at the end of the volume, lacking in the first edition, greatly facilitates reference.

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M. W.: It was a happy idea to collect in one volume the scattered articles of the late Professor BRUNN which bear upon Roman and Etruscan monuments (Teubner). These range in date from 1844-85 and the greater number are in Italian. We are told in the preface that Brunn was averse to a German translation being made of these, holding that the same idea must be expressed differently in German and Italian, the latter language demanding a more concrete and elementary exposition. The articles have to do with various sarcophagi, vases, mirrors, bronzes, terra-cottas, and Etruscan paintings, and the clear and genial interpretation of the distinguished critic has an abiding value, even though his views may long since have been accepted, modified or abandoned. No one better appreciated than Brunn the value of ancient works of art for the illustration of ancient literature, and to read his work is to be convinced that archae-

ology and philology are mutually interdependent. A sarcophagus may admirably elucidate the epithalamion of Statius, an altar with its sculptured sides may furnish a commentary to an ode of Horace, and the interpretation of Ovid would greatly gain in vividness, if the classical editor were better acquainted with the ancient monuments. On this ground, if for no other, we recommend the perusal of this book to the classical philologist, and from this point of view the following articles are especially interesting: *Sarcosago rappresentante ceremonie nuziali*, *Die Ara Casali*, *Giunone Lucina*, *Vatikanischer Relief-pilaster*, *I monumenti degli Aterii*. The book is richly illustrated, and most of the illustrations are good, though some of them lack distinctness.

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Thanks are due to Messrs. Lemcke & Buechner, 812 Broadway, New York, for material furnished.

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## ERRATA.

P. 105, l. 23 from top, read *σεισμῶν*.  
 P. 113, l. 15 from top, read *ōte-toi* <*de là*>.  
 P. 232, l. 2 from top, read *laudaveris*.  
 P. 232, l. 23 from top, read "Cicero de Oratore."

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